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JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

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The Sixty-second Ordinary General Meeting was held on Wednesday, the 23rd January, 1878, PROFESSOR ABEL, C.B., F.R.S., President, in the Chair.

The preliminary business of the evening having been disposed of,

PROFESSOR ABEL rose and said: Gentlemen, in retiring from the chair to which you did me the honour to elect me twelve months ago allow me in the first place to return to you my cordial thanks for the indulgence with which you have received my earnest efforts to fulfil, in a manner worthy of you, the duties with which you entrusted me; and in the second place to express to the Members of the Council, and the officers of the society, my hearty thanks for their able co-operation in the performance of my duties, whereby the work of the President has been made throughout most agreeable and at the same time by no means arduous. And one word of special thanks, not only on my behalf but on that of the Council generally, is due to the gentleman who has during nearly the whole period of my office occupied the position of acting secretary. The zeal and industry, combined with happy tact, great discrimination, and excellent judgment with which Mr. Langdon has performed the duties of secretary to this now important Society have shown him to be pre-eminently fitted for such a position. He has rendered the Society valuable service, and sorry must we all feel that he is about to quit the secretaryship, and that circumstances

have rendered his retirement at once from the position, peremptory. I therefore feel sure you will be with me entirely when I return my thanks to Mr. Langdon for the manner in which he has performed the duties of secretary during my term of office. And now I have but one more, and not the least pleasing, duty to perform, viz:—cordially to welcome to the re-occupation of this Chair our first President, Dr. C. W. Siemens, one of the most distinguished as well as most zealous founders of this prosperous Institution. (Loud applause.)

Dr. Siemens, the President elect, having taken the chair,

Mr. Latimer Clark rose and said: I am sure, Mr. President and Gentlemen, you will join cordially with me in offering a vote of thanks to Professor Abel for his unremitting attention to the interests of this Society. As one of the Council I am able to bear testimony to the constant and thoughtful attention he has given to our concerns, and the readiness with which he has followed every suggestion of the Council or others for furthering the welfare of the Society and promoting the interests of our profession. From his high position and influence in the scientific world he has been able to render us important services, as, for example, among other things, in obtaining for the Society a complete set of all Patents connected with electricity and magnetism from the earliest date to the present hour, a collection which must be of the highest value to students of electricity. I need not say a single word about his conduct in the Chair; a better Chairman we could not have, and never have had; therefore I am confident you will all join in a hearty and cordial vote of thanks to Professor Abel for the valuable services he has rendered to this Society.

Major Webber: It is your pleasure, and a great pleasure to me, that I should be your representative in seconding the vote which Mr. Latimer Clark has proposed. There is one thing we ought to congratulate ourselves upon in connection with our young Society, which is, that it has brought around us, and into our ranks, men so distinguished as Professor Abel; men who for years before our existence, made themselves celebrated in the scientific world; and, as a result of this, we may congratulate, not only the Society but the telegraphic world, upon the researches, and value of those

researches, which we know Professor Abel has so successfully made with respect to the insulating properties of gutta percha. I beg to second the proposition of Mr. Latimer Clark, which I am sure will be responded to with the greatest cordiality.

The motion was carried by acclamation.

The President: (Addressing Professor Abel.) The most pleasing, as it is the first duty I have to perform, is to express to you the unanimous thanks of the Society of Telegraph Engineers for the great services you have rendered them during your term of office, and I much regret that the rules of the Society render it impossible for the President to occupy this seat for more than one year consecutively, otherwise there could not possibly have been a more worthy representative of this Society for the ensuing year than yourself.

Professor Abel: I thank you most cordially for the kind manner in which you have been pleased to recognise the services I have endeavoured to render during my term of office. I may be allowed to add that the close of my term of office as President will not terminate my active interest and active work connected with this Society. I hope still to be as useful to the Society as it has been my endeavour to be up to the present time. (Loud applause.)

The President: It is now my duty to announce that this society has had a presentation made to it of a most valuable portrait of our retiring President—a portrait which is presented to the Society by our friend and indefatigable Honorary Secretary, Colonel Bolton. The portrait is before you, and speaks for itself. I am quite sure you will all feel that a portrait of our retiring President forms a valuable addition to our collection. I call upon you to express a hearty vote of thanks to our Honorary Secretary for this very valuable gift.

The vote of thanks was carried by acclamation.

The President then delivered his Inaugural Address, as follows:

Gentlemen,

Six years have now elapsed since I had the honour of addressing you as first President of the Society of Telegraph Engineers.

The hopes which I then expressed as to the probable development of the Society have been fully realized under the able guidance of my successors in office, combined with the active and ever-increasing support of our honorary secretary Colonel Bolton.

At the time I addressed you first, the Society was composed of only 110 Members of every description. This number increased during my term of office to 353, whilst it has now reached up to nearly 1,000 Members, a number quite sufficient I should say to insure a continuance of its prosperous career.

The six volumes of Transactions issued by the Society since its origin are proof of its activity as a scientific institution, whilst its status has been much advanced through the establishment of a scientific library, bequeathed by the late Sir Francis Ronalds, and now in the trust of the Society, containing a most valuable record of all publications having reference to the advancement of Telegraphy. In order to make this collection of permanent value it will be necessary to complete the record always up to date, a duty which I trust will be faithfully and well discharged by the officers of the Society.

In reviewing the progress made in Telegraph Engineering during the last few years, I propose to notice in the first instance the subject of Duplex and Quadruplex Telegraphy, which has recently much occupied the attention of the Telegraph Engineer. Duplex Telegraphy has been known and practised to a very limited extent since 1854, when it was first announced by C. A. Nyström of Öreboro, Sweden, and by Dr. Gintl, of Vienna, and carried out practically by Frischen and Dr. Werner Siemens. Although quite successful in some of the applications made at that time in Germany, in Holland (between Amsterdam and Rotterdam), and in this country under my own superintendence between Manchester and Bowden, telegraphy itself had not advanced sufficiently to call for an application of this invention upon a more extended scale, and it has only met with favour on the part of telegraph administrations since its re-introduction to public notice by Mr. Stearn, of Boston, in 1872, who improved, however, upon the original arrangement by balancing the discharge from the line by the discharge from an arrangement of condensers. Another important advance in

duplex telegraphy has been made by Mr. Louis Schwendler, who by the application of an improved Wheatstone Bridge arrangement has produced the means of readily adjusting the effect of the neutralizing current during the working of the instrument, and has carried duplex telegraphy into effect with great advantage upon the long lines of India, with which he is connected.

The quadruplex telegraph, which may be considered to have been theoretically introduced by Dr. Stark, of Vienna, in 1855, and contemporaneously by Dr. Boscha of Leyden, has been developed by Mr. Edison of New Jersey, U.S., and has been for some time established upon the line between New York and Boston, under the superintendence of Mr. Prescott, the engineer of the Western Union Line. In this system the principle of duplex telegraphy is combined with the equally well-known system of producing different effects by currents differing in strength, and it is, indeed, not difficult to conceive that by further combinations of the same nature six or eight pairs of instruments may be worked simultaneously and independently through one and the same conductor. The success of these improved methods of transmission depends almost entirely upon the perfect insulation and undisturbed condition of the line-wire, a subject which has yet to receive much attention on the part of the Telegraph Engineer.

Our attention is next arrested by the great novelty of the day, the Telephone.

This remarkable instrument owes its origin to the labours of several inventors.

In the year 1859 the late Sir Charles Wheatstone devised an arrangement by which the sounds of a reed or tuning-fork, or a combination of them, could be conveyed to a distance by means of an electric circuit, including at both stations a powerful electro-magnet. In striking any one of the tuning-forks differential currents were set up which caused the vibration of the corresponding tuning-fork at the distant station, and thus communicated the original sound. In 1862 Reiss enlarged upon this ingenious suggestion in attempting to convey the varying vibrations of a diaphragm agitated by atmospheric sound-waves. His apparatus consisted of a parchment diaphragm with a thin platinum wire

attachment set into vibration by sound, which caused a series of contacts to be made, and the galvanic currents thus sent through an electric circuit, produced sounds by the making and unmaking of an electro-magnet at the distant station.

This instrument transmitted currents only of equal intensity, and produced therefore sounds of equal calibre, distinguishable only by their periods. It was thus capable of transmitting simple tunes, but was quite incapable of transmitting the human voice with its innumerable modulations of sounds, varying both in period and intensity.

These defects in the instrument of Reiss have been remedied by Mr. Edison, who, by establishing contacts through the medium of powdered plumbago, has succeeded in transmitting galvanic currents varying in intensity with the amount of vibration of the diaphragm.

As another step towards the accomplishment of the perfect transmission of sound, I should mention also the logograph, or recorder of the human voice, which Mr. William Henry Barlow, F.R.S., a Member of our Society, communicated in a paper to the Royal Society, on the 23rd February, 1874.

In adding a contact arrangement to the recording pencil of Mr. Barlow's instrument, the message could obviously be transmitted to a distance to be recorded there either by graphical or audible signals.

The beautifully simple instrument of Professor Graham Bell, of Cambridge, U.S., must be regarded as a vast step in advance of all previous attempts in the same direction. In making the diaphragm of iron, and having recourse to Faraday's great discovery of magneto-induction, Mr. Bell has been able to dispense with the complication of electrical contacts and batteries, and to cause the vibrations of the diaphragm imparted by the voice to be accurately represented in strength and duration by electrical currents, thus producing the marvellous results of setting up analagous vibrations in the diaphragm of the receiving instrument, which, though weaker than the vibrations imparted to the transmitting diaphragm, so closely resemble them as to repeat the quality of voice which causes the original vibrations.

The currents transmitted are so minute as to escape observation by the most delicate galvanometer, as the magnetic needle, however light, must be too sluggish to be moved visibly by such quick impulses, and it requires an electro-dynamometer of exceeding sensitiveness to bring them into evidence. The rapidity with which these reversing currents follow each other can be accurately determined in transmitting the sound of a high-pitched tuning-fork, and Mr. Köntgen concludes from experiments he has made in this direction that not less than 24,000 currents can be transmitted in one second. We here detect a rapidity of electrical transmission far exceeding our most sanguine expectations in endeavouring to increase the rate of transmission of telegraph instruments by mechanical means, thus opening out a new field for the inventive faculties of the Telegraph Engineer.

The telephone is no doubt capable of great improvement, which should chiefly be directed towards increasing the relative amount of vibration of the receiving diaphragm.

Improvements will doubtless be directed also towards the accomplishment of simple methods of recording the audible messages received, which has already been attempted by Mr. Edison, and of carrying out such accessory objects as the ringing of call bells, and the transmission of the sound-waves through additional circuits.

Considering the minuteness of the electrical impulses and their high electro-motive force, it seems probable that they will be capable of being transmitted to very great distances through conductors of comparatively small dimensions, provided only that those conductors are not subjected to the disturbing influence by induction of currents flowing through adjoining wires. It is well known that owing to these disturbing currents the telephone cannot be worked through a wire suspended with other wires upon posts in the ordinary way, and it will be necessary to devise other means of carrying the telephone conductor.

The system of suspended line-wires now generally in use is open to many grave objections. Atmospheric electricity frequently causes such disturbances as to interrupt the working of long lines for hours at a time, and the most perfect lightning dischargers do

not always prevent damage to the working instruments, when atmospheric discharges of electricity into the line-wire take place. Again, the mutual induction between parallel line-wires and the leakage from one wire to another through the supporting poles are a permanent source of trouble in working telegraph instruments, and this difficulty increases as we advance from the simple needle or recording instrument to the more refined duplex or quadruplex system, to the mechanical transmitter or the telephone.

Again, it happens that not unfrequently suspended line-wires are thrown down, causing the almost entire cessation of telegraphic communication for days, in the event of a great gale or snowstorm, interruptions which are quite incompatible with the idea that the Electric Telegraph has become a great public institution.

The remedy for these interruptions is undoubtedly the underground line-wire system. This was first tried in Germany upon an extended scale in 1848-49, but was given up in favour of the suspended line in consequence of the want of experience in manufacture and imperfect protection afforded to the gutta-percha covered copper wire. Since then it has been largely used in this country for underground communication in cities, also for aerial lines, by suspending a bunch of the insulated conductors by steel wires in the air, as we see them supported on the house-tops of this metropolis. The German Telegraph Administration, under the able direction of Dr. Stephan, has within the last year or two again resorted to the application of the underground conductor for long lines. A representative cable of what it was intended to lay was put down in 1876 between Berlin and Halle, a distance of 120 English statute miles. The success of this line induced his Government to lay down last year, multiple cables between Berlin and Cologne, and Berlin, Hamburg, and Kiel, an aggregate distance of 600 miles, and further extensions are in course of execution. These cables consist of seven separate conductors, each insulated with gutta-percha, surrounded with a complete iron sheathing and a double outer covering consisting of hemp steeped in asphalte, producing altogether a flexible cable of $1\frac{1}{8}$ inch outer diameter, which is laid along railways or roads at a depth of about 3 feet below the ground.

Great precautions have been adopted to prevent failure of these newly established lines, whilst the ease with which these comparatively long circuits can be worked by means of every description of instrument, including the telephone, and their perfect immunity from atmospheric disturbances, will lead, I venture to predict, to the gradual substitution of underground wires for suspended line-wires for all the main arteries of the telegraphic system.

In submarine telegraphy no startling feat of novelty can be reported, although steady progress has recently been made in improving the manufacture of the insulated conductor, in the attainment of an increased rate of transmission through long distances, in the outer protection given to the insulated conductor, and in the vessels and other appliances employed for submerging and repairing deep-sea cables.

The conductor almost universally adopted in the construction of submarine cables has been a strand of seven copper wires, covered with three thicknesses of gutta-percha, with intervening layers of a fusible resinous compound. In the case of the Direct United States Telegraph Company's Cable, the copper conductor consists of one large central wire of 0·090 in. diameter, surrounded by eleven smaller ones of 0·035 in. diameter. By this construction an increase of about 10 per cent. of conductivity is obtained for a given outer diameter, which increase has been found to exercise an important effect upon the rate of transmission through the cable.

The careful selection of the insulating material employed has also an important influence upon the rate of transmission through long cables, as it is found that different kinds of gutta-percha behave very differently in this respect. India-rubber has, it is well known, considerably less inductive capacity than gutta-percha, and appears on this account the preferable material, but its application to the conductor, without the risk of faults and of gradual changes in the condition of the material, is beset with considerable practical difficulty which has as yet limited its application. Compounds of india-rubber and gutta-percha, with other materials such as shell-lac, paraffin, and bitumen, have been proposed from time to time with promising results, but it has been impossible hitherto to give to such compounds all the properties necessary in the dielectric

substance covering the conductor, viz., a low inductive capacity and high insulation, coupled with considerable toughness and permanency at all ordinary temperatures and the requisite plasticity at higher temperatures.

The supply of gutta-percha has hitherto been sufficient for the demand, but a large extension in the use of insulated conductors both by sea and land will, it may be apprehended, outrun the supply, and it is well on this account that we should steadily fix our attention upon such compounds as are likely to furnish a suitable substitute. Regarding a continued supply of gutta-percha and india-rubber, it is satisfactory to observe that the Indian Government have turned their attention seriously to the question of making plantations of trees bearing these gums, chiefly in the Malay Peninsula, under the able direction of Sir Joseph Hooker, and of Dr. Brandes, the Director of the Forest Department in India. It is to be hoped that by these wise measures a continued supply of these invaluable materials will be secured, while their quality for insulating purposes will probably be improved by means of cultivation.

The outer covering now generally applied to shallow-sea cables consists of a sheathing of iron wire covered with a double layer of hemp steeped in asphalte, and applied to the cable in a heated condition, and this, if properly carried out, affords very efficient protection for the iron sheathing against corrosion.

In the construction of deep-sea cables, steel wires are generally used, each wire being covered in the first instance with jute with a view to reduce the weight of the cable. This construction affords the advantage of lightness combined with strength, and thus facilitates the operation of submerging the cable, but is objectionable, inasmuch as it affords no complete metallic sheath against the inroads of the Teredo and Xylophaga to the core, and, in the case of a cable having to be raised from considerable depths, it is apt to untwist, and run itself into kinks at the bottom.

The use of a light cable for deep seas has been ably advocated by some Electricians, and its adoption has the one great argument in its favour, that its first cost is much below that of a strong cable; on the other hand the risk incurred in successfully submerging such a cable is much greater, and in the case of a fault appearing

in deep water it will be hopeless to bring the light cable to the surface for the purpose of repair. It is possible that the manufacture of cables will eventually be made a matter of such absolute certainty that the case of faults making their appearance in submerged cables may be left entirely out of consideration, but in the meantime Telegraph Companies have given the preference, and wisely so, I think, to a cable which though more costly than its light competitor affords a greater security to their property in case of an accident or a fault.

Whilst the art of submerging deep-sea cables, involving, as it does, problems of very considerable scientific and practical interest, has latterly received the attention of this Society, but little discussion has as yet taken place of the best means for effecting the repair of cables after submersion.

The important primary condition towards effecting the repair of a submerged cable is that its general insulation should be perfect, without which it would be impossible to determine the position of a break or fault with any degree of accuracy. Another important condition is the possession of a cable-ship furnished with special facilities for manœuvring. The old practice of using ordinary steamships of the mercantile marine appears very primitive and objectionable, as such vessels are ill adapted for going astern, are not steady when laden with cable and armed with heavy deck machinery, and are incapable of turning or maintaining their position against a side-wind unless going nearly full speed, whereas the cable ship should be capable of effecting these operations independently of any onward motion. The paying-out and hauling-in machinery, the tackle for fixing and lighting buoys, the arrangements for sounding, and the construction of grapnels capable of finding, cutting, and holding the end of deep-sea cables, are also matters influencing greatly these delicate operations, upon which the permanent success of submarine telegraphy must mainly depend.

The transmission of telegraphic messages through long submarine cables is a subject which was at one time involved in great practical difficulty owing to the retardation, by lateral induction, experienced by the electrical current in its transit. It is to our past President

Sir William Thomson that we are indebted for a solution of this difficulty, through the application of his celebrated mirror instrument, which is capable of revealing to the eye extremely slight remnants of electric waves, readily transferred by means of a human relay to ordinary recording instruments, and for the further introduction of his syphon recording instrument, by which those slight currents are rendered in a written code. This latter instrument, however, is of a somewhat delicate and complicated nature, and it would be desirable if its place could be taken by a relay of extreme sensitiveness, coupled with ordinary recording instruments worked by local circuits, the accomplishment of which result we may anticipate before long, considering the great improvements that have been effected in the construction of polarized relays.

Although this country has from the first taken a prominent part in the invention and development of the electric telegraph, and is still the seat of oceanic telegraphic enterprise, almost to the exclusion of other countries, it has lately been asserted that other countries, and especially the United States, are now taking the lead in telegraphic improvement, and it behoves us to inquire whether such an allegation is founded on fact, and if so whether it is attributable to indolence on our part or to circumstances beyond our control. Steady progress has, as I have shown, been made by us up to the present day in the instruments and other appliances used in telegraphy, but it cannot be denied that the more startling innovations of recent days have chiefly emanated from the United States, the only civilized country in which, as it happens, internal telegraph communication is still in the hands of private companies. Is it, it may be asked, this open competition which has stimulated the American inventor to bring forth duplex and quadruplex telegraphy, the telephone, and other innovations? I incline to the belief that the open competition for public favour does act as a powerful stimulant to invention in the United States, a stimulant which was equally active in this country in producing a variety of novel instruments, at the time prior to the purchase of the telegraphs by the Government.

In frankly giving expression to this opinion, I do not mean to call in question the wisdom of the policy which dictated the

purchase, on Public grounds, of the telegraphs by Government. Through it we have obtained a uniform and moderate tariff, an extension of the telegraph system to minor stations (although the number of stations opened in this country does not yet exceed that provided in the United States, being in the one case a station for every 5,607, and in the other for every 5,494 inhabitants*), and a better guarantee for the secrecy of messages. The growing connection between the telegraph systems of this and other countries would have compelled by degrees the active intervention of the Government, which alone could arrange effectively, with the telegraph administrations of other countries, general questions of tariff and modes of working. The triennial meeting of the Telegraph Conference will, as you are aware, take place this year in London, and will enable us to judge more fully of the beneficial results of co-operation between the telegraphic systems of the world.

The conference does not interfere, however, with matters of technical import, such as the construction of lines and improvement of instruments for working the same, in which we are chiefly interested, and it is a question worthy of consideration whether the Acts of Parliament of 1868-69, by which the Government Department of Telegraphs was created in this country, do not go beyond the limits necessary to insure a well-regulated public service in taking the construction as well as the working of the lines out of the hands of public enterprise. They give for instance to the Department the faculty of purchasing letters patent, whereby an interest is created in favour of particular instruments, to the prejudice of others of perhaps equal merit, and such a course is by no means calculated to stimulate invention.

The erection of lines for local and private purposes is an important branch of telegraphy which I submit should have remained entirely outside the scope of a Public Department, in order that competition might have a free opportunity of developing such applications, as is the case in the United States, where private and circular telegraphy is undoubtedly in advance of other countries.

* See Statistical Tables in the *Iron Age* for 14 June, 1877.

In venturing upon these observations, I wish it to be clearly understood that I do not mean to insinuate that the engineers and other officers of the telegraph administration have not been most anxious to secure the greatest amount of public benefit, or that they have been remiss in their endeavours to improve the condition of the line-wire and working instruments upon which the public service depends.

Great improvements have indeed been recently made by the Postal Telegraph Department in the rate of working of Wheatstone's automatic circuits, and in the employment of fast-speed translators or repeaters, as is proved by the following data, for which I am indebted to our Vice-President, Mr. W. H. Preece. For instance, it has been found that the insertion of one of the new fast-speed translators at Dublin has more than doubled the rate of working between London and Cork, and the insertion of one of these relays at Anglesea has improved the rate of working between London and Dublin about 50 per cent.

As an indication of the rate at which messages can be transmitted, it appears that the Queen's Speech, containing 801 words, was sent to Leicester in 4m. 28secs., being at the rate of 179 words per minute. The quickest rate at which it was sent by key was between London and Reading, where it occupied seventeen minutes, or at the very high speed of 47·1 words per minute.

It is perhaps interesting to remark that on the first night of the Session, over 420,000 words were actually transmitted from the central station, and over 1,000,000 words were delivered in different parts of the country.

The quadruplex system of telegraphy continues to be worked with very satisfactory results between London and Liverpool, and it has quite quadrupled the power of the one wire to carry messages. The highest number of messages transmitted in one hour has been 232; about 200 per hour have frequently been sent.

The system of duplexing Wheatstone automatic circuits is gradually extending, and on the Leicester wire which carried the Queen's Speech at the rate named, messages were being transmitted in the opposite direction by the duplex arrangement at the same time.

In submarine telegraphy ample scope still exists, as I have endeavoured to show, for the ingenuity and enterprise of the telegraph engineer; but here again the free exercise of these faculties is threatened, not by legislative action, but by a powerful Financial combination. It is intended by this combination to merge the interests of all oceanic and international lines and the construction of new lines into one interest; but it seems hardly probable that such a monopoly will be able to maintain itself in the long run against that irrepressible spirit of British enterprise, which, though languishing at the present time of unparalleled depression, is likely to reassert itself before long.

Electricity has hitherto rendered service as the swift agency by which our thoughts are flashed to great distances, but it is gradually asserting its right also as a means of accomplishing results where the exertion of quantitative effects are required. Much has been said about the application of electricity for producing light, and the French Company Alliance, as well as the Gramme Company, have, it is known, for some years been establishing magneto-electric apparatus to illuminate the lighthouses upon the French coast, and for galvano-plastic purposes.

By an ingenious combination of two magneto-electric machines, with Siemens armatures, Mr. Wilde, of Manchester, succeeded in greatly augmenting the effects produced by purely mechanical means, but the greatest impulse in this direction was given in 1866-67 by the introduction of the dynamo-electrical principle, which enables us to accumulate the current active in the electric circuit to the utmost extent permissible by the conductive capacity of the wire employed. Dr. Tyndall and Mr. Douglass, chief engineer to the Trinity Board, in reporting lately to the Elder Brethren upon the power of these machines and their applicability to lighthouses, gave a table showing that a machine, weighing not more than 3 cwt., is capable of producing a light equal to 1,250-candle power per horse power expenditure of mechanical energy. Assuming that each horse-power is maintained with an expenditure of 3 lbs. of coal per hour (which is an excessive estimate) it would appear that one pound of coal suffices to maintain a light equal to 417 normal candles for one hour. The same amount of light would be pro-

duced by 139 cubic feet of gas of 18-candle power, for the production of which 30 pounds of coals are consumed. Assuming that of this quantity, after heating the retorts, &c., 50 per cent. is returned in the form of gas-coke, there remains a net expenditure of 15 pounds of coal in the case of gaslighting to produce the effect of one pound of fuel expended in electric lighting, or a ratio of 15 to 1 in favour of the latter. Add to the advantages of cheapness in maintenance, and of a reduced capital expenditure in favour of the electric light, those of its great superiority in quality and its freedom from the deleterious effects of gas in heating and polluting the atmosphere in which it burns, and it seems not improbable that it will supersede before long its competitor in many of its applications. For lighthouses, for military purposes, and for the illumination of large works and public buildings the electric light has already made steady progress, while for domestic applications the electric candle proposed by Jablochkoff, or modifications of the same, are likely to solve the difficulty of moderating and distributing the intense light produced by the ordinary electric lamp. The complete realization of all the advantages of the electric light remains, however, a problem to be solved, and it would be extravagant to expect from applications on a small scale such as have hitherto been made, anything like the amount of relative advantage indicated by theory.

The dynamo-electric machine has also been applied with considerable success to metallurgical processes, such as the precipitation of copper in what is termed the wet process of smelting. The effect of one horse-power expended in driving a dynamo-electric machine of suitable construction is to precipitate 1120 pounds of copper per 24 hours, equivalent to an expenditure of 72 pounds of coal, taking a consumption of 3 lbs. of coal per horse-power per hour.

Electrolytic action for the separation of metals need not be confined however to aqueous solutions, but will take perhaps an equally important development for the separation, while in a state of fusion, of the lighter metals, such as aluminium, calcium, and of some of the rarer metals, such as potassium, sodium, &c., from their compounds. Enough has been shown by Professor Himly, of Kiel,

and others, to prove what can be done in this direction, although there remain practical difficulties (chiefly the rapid destruction of the vessels containing the fused masses), the removal of which will require patient perseverance, but is not likely to prove of an insuperable character.

In an inaugural address which I had occasion to deliver to the Iron and Steel Institute a twelvemonth ago I called attention to another application of the dynamo-electric current, that of conveying mechanical power, especially the power of such natural sources as waterfalls, to distant places, where such power may find useful application.

Experiments have since been made with a view to ascertain the percentage of power that may thus be utilized at a distance, and the results of these experiments are decidedly favourable for such an application of the electrical conductor. A small machine, weighing 3 cwt. and entirely self-contained, was found to exert 2·3 horse-power as measured by a Prony's brake, with an expenditure of five horse-power at the other end of the electric conductor, thus proving that above 40 per cent. of the power expended at the distant place may be recovered. The 60 per cent. lost in transmission includes the friction of both the dynamo-electric and electro-motive engines, the resistance of the conductor, and the loss of power sustained in effecting the double conversion. This amount of loss seems considerable, and would be still greater if the conductor through which the power were transmitted were of great length and relatively greater resistance; but on the other hand it must be remembered that the power of a natural motor is obtained without expenditure of coal, and that a small caloric motor which the electric motor is intended to supplant is inconvenient and very extravagant in fuel. The electric motor presents moreover this great advantage, that it requires hardly any installation, and would be available at any time by merely closing the electric circuit without incurring the risk and inconvenience inseparable from steam and gas engines.

Without considering at present the utilization of natural forces, let us take the case of simply distributing the power of a steam-engine of say 100 horse-power to twenty stations, within a circle of

a mile diameter, for the production of both light and power. The power of 100 horses can be produced with an expenditure of 250 pounds of coal per hour if the engine is constructed upon economical principles, or of

$$\frac{250}{20} = 12{\cdot}5 \text{ lbs}$$

per station. In the case of the current being utilized for the production of light

$$2{\cdot}3 \times 1200 = 2760,$$

or say 2,000-candle power, are producible at the station, whereas if power is desired 2·3 horse-power may be obtained, in both cases, with the expenditure of 12·5 pounds of coal, representing a penny an hour for cost of fuel, taken at fifteen shillings a ton. The size of the conductor necessary to convey the effect produced at each station need not exceed half an inch in external diameter, and its cost of establishment and maintenance would be small as compared with that of gas or water pipes for the conveyance of the same amount of power

Electricity, which in the days of Franklin, Galvani, Volta, and Le Sage, was regarded as an ingenious plaything for speculative minds, and did not advance materially from that position in the time of Oersted and Ampère, of Gauss and Weber, and not indeed until the noon-day of our immortal Faraday, has, in our own times, grown to be the swift messenger by which our thoughts can be flashed, either over-land or through the depths of the sea, to distances, circumscribed only by terrestrial limits. It is known to be capable of transmitting, not only language expressed in conventional cypher, but facsimile copies of our drawings and handwriting, and at the present day even the sounds of our voices, and of resuscitating the same from mechanical records long after the speaker has passed away. In the Arts it plays already an important part through the creation by Jacobi of the galvano-plastic process, and in further extension of the same principle it is rapidly becoming an important agent in the carrying out of metallurgical processes upon a large scale. It has now appeared as the formidable rival of gas and oil for the production of light, and, unlike those inferior agents, it asserts its higher nature in rivalling solar light for the produc-

tion of photographic images; and finally it enters the ranks as a rival of the steam-engine for the transmission and utilisation of mechanical power.

Who could doubt under these circumstances that there remains an ample field for the exercise of the ingenuity and enterprise of the Members of that Society I have just had the honour of addressing?

COLONEL CROSSMAN, C.M.G., R.E.: I feel that I am only expressing the unanimous feeling of the Meeting, when I state that we have listened with the greatest interest and pleasure to the most able Address of the President; and I am sure that so long as we have the pleasure of listening to such inaugural addresses as we have the advantage of hearing at our annual meetings there can be no fear for the future of this Society. On the merits of the Address we have just heard, treating as it does so comprehensively of all matters, philosophical and practical, it is not my duty to dilate, but I feel convinced, Gentlemen, that you will unanimously resolve "That the Address of the President be, with his permission, printed and circulated among the Members of the Society."

Mr. GRAVES: I beg to be permitted the pleasure of seconding the motion just proposed. I do not think it would become me to occupy your time with remarks of my own, and I will not do so further than to state that I had the pleasure of being sent to represent the telegraph administration of this country at the experiments recently carried out at Kiel in connection with the working of the underground lines to which the President has alluded. I will add my humble testimony to the complete success of the efficient working of those lines. There is one thing the President did not tell you, that is, the system is rather costly, and there are financial reasons why it has not been carried out to the extent that might have been anticipated. With reference to the allusions in the Address to the monopoly of telegraphs possessed by the State, I think in the position in which I stand the less I say the better. All I have to remark is that there is a good deal to be said on the other side. I will conclude by adding my testimony to the great value and interest of the Address as a compendium of the past history and present position of the science of telegraphy, and I heartily second the vote of thanks to the President.

The motion having been put to the Meeting by Professor Abel, was carried unanimously.

The PRESIDENT: I beg to thank those Gentlemen who have spoken with regard to my Address, and you Gentlemen for the kind manner in which you have received the same; and I hope during my term of office that I shall continue to receive your support in the discharge of those duties which you have done me the honour to entrust to me. Those duties, I am afraid, I shall find it somewhat difficult to discharge, owing to a good many other engagements which press upon me just now, and I therefore speak candidly, and with due feeling of the weight that rests upon my shoulders, when I ask your kind indulgence during my term of office. However, as my interest in the Society is unabated, I will do what in me lies to conduct the business of this Society in such a way as not to interrupt its course of progressive benefit. (Hear, hear.) This year is an interesting one on account of the meeting of the Telegraph Conference in London, and it is a duty, as well as a pleasure, that devolves upon us to entertain those gentlemen, representatives of the telegraph systems of nearly the whole world, who will come amongst us, and will be glad to see our ways of doing things, and to shake hands with this Society, which is now known throughout the length and breadth of the civilised world. (Hear, hear.) The Secretary reminds me that a subscription has been called for with the view of constitutiug a fund for defraying certain expenses which the Society, as a whole, will be put to in regard to that Conference, and that subscriptions to the amount of £340 have already been received. If any gentlemen wish to increase the amount it is quite within their power to do so.

On the motion of Mr. C. V. Walker, F.R.S., seconded by Mr. W. T. Ansell, it was then proposed and carried by acclamation,

"That the best thanks of this Society be given to the Institution of Civil Engineers for their continued free and cordial hospitality to the Society of Telegraph Engineers."

The PRESIDENT having announced the desire of the Council to

receive applications for the post of Secretary from such Members and Associates of the Society as might be willing to undertake the duties of that office,

A vote of thanks to the retiring Acting Secretary for the services rendered by him to the Society during his term of office was proposed by Mr. T. P. Bruce Warren, seconded by Mr. Donovan, and carried unanimously.

Mr. EDMUNDS, Junr., exhibited a new form of battery, a full description of which will be found at page 60: and Mr. BRITTLE exhibited the dynamo-electric light, when the Meeting adjourned till the 13th of February.

The Sixty-third Ordinary General Meeting was held on Wednesday, February 13th, 1878, Professor ABEL, F.R.S., Past-President, in the Chair.

The CHAIRMAN: It is my painful duty to announce to the Members the death of an Honorary Member—Mr. Samuel Carter—formerly solicitor to the London and North Western Railway, and subsequently, for many years, solicitor to the Midland Railway. Mr. Samuel Carter was a valuable Member to us, and highly esteemed by all who knew him personally, but we have especial reason to regret his loss, and to bear respect for his memory, from the fact that it is to him entirely we owe the acquirement of that most valuable property—the Ronalds' Library. The credit is due to Mr. Samuel Carter, to whom that library was unreservedly bequeathed, that we have become possessors of that important collection, which is not only valuable in itself pecuniarily, but which really gives the Society additional importance, and I am sure you will all share in the feeling of regret at the death of Mr. Carter.

With reference to the announcement made at the last meeting that the office of Secretary to the Society would be filled up, I have to report to the Members, on behalf of the Council, that they have carefully considered the merits and claims of the various candidates who have applied for that appointment, and that their choice has fallen, after careful deliberation, upon Mr. F. H. Webb, who has been appointed Secretary to the Society, and will immediately take office in that capacity.

I will now call upon Mr. Preece to deliver his lecture upon the American Telegraph System.

Mr. W. H. PREECE: Mr. President and Gentlemen, It will be in the recollection of most of the Members present that in the spring of last year Mr. Henry Fischer, the Controller of the Central Telegraph Station, and myself, were appointed by the Government to proceed to America, and there to inspect and report upon the telegraphic system of that country. We left Liverpool in the Cunard steamship "Abyssinia" on the 4th of April, and after an

extremely pleasant time on the ocean we arrived at New York on the 14th of April. Now, if there are any Members present who are wearied with work, or anxious for rest, or are desirous of peace and comfort—if they have good stomachs and good sea-legs, there is nothing on the face of the earth so calculated to put them right as to take a trip across the broad Atlantic. We arrived fit and well at New York, and we were received there, as I am sure every Member of this Society will always be received there, in a most cordial and hearty way. Everything we could possibly require was placed before us by the different Telegraph Companies; every officer of those Companies did his utmost to meet our wishes; and during our whole stay on that great Continent we never had occasion to look back upon anything with regret or to experience unpleasantness of any sort or kind.

Now, Buckle wrote a book, in which he attempted to show that the characteristics of different nations were to a certain extent dependent upon the character of the climate of the country. We are all of us aware of the enormous energy—the go-aheadness of our good friends on the other side of the Atlantic. There is no great difficulty in attributing those characteristics to the beautiful air and bracing atmosphere of that portion of the world. As regards our two selves, we certainly, in the short time at our disposal, did an amount of work which, when we contemplate, makes us shudder.

Our tour through America was not a great one when you look at the map. We arrived at New York and spent three or four weeks in that city in making ourselves thoroughly acquainted with the telegraph systems there. This was in itself no light task, because we had to unlearn a good deal that we had learnt in England and had to start upon an almost entirely new plan. We had to make ourselves acquainted not only with the engineering details but with the commercial details, the modes of transmission, the tariff, the sources of revenue, and with the rules and regulations of the Telegraph Companies there. This of course took a considerable amount of time. After spending sufficient time—about four weeks —in New York, we then travelled to Philadelphia, Baltimore, and Washington; thence we went up the Valley of the Potomac across the Alleghany Hills to the Valley of the Ohio and Cincinnati,

then on to Indianopolis, and thence to Chicago. From Chicago we wandered through Canada to Niagara. There is not much in the telegraph way to be seen at Niagara, but we determined to see what was to be seen there of Nature's ways as thoroughly as we did at New York of telegraphic ways. From Niagara we went to Toronto, down the St. Lawrence to Montreal and Quebec, and thence across the White Mountains to Portsmouth, from thence to Boston, and thence by the Fall River to New York again.

Though our journey occupied several weeks, and though we travelled night and day over thousands of miles, still, when you compare our journey with the enormous continent itself, the space we covered was an extremely small one. Nevertheless we saw all we wanted to see of the telegraph system of the country, and learnt all we wished to know of that system. We moreover got thoroughly imbued with Yankee notions about hotel living, which to my mind far surpass European notions. If you want to know what hotel living is, go to the Fifth Avenue Hotel at New York, or the Palmer House at Chicago, or the Grand Union at Saratoga, where 1,800 guests are housed, who are attended to by 500 servants, and where therefore 2,300 mouths are fed every day without half the fuss you see in the dirty little hotels in some of our country towns. Then again, at these hotels, if you arrive hungry they can feed you, and naked they can clothe you. The entrance halls are surrounded by shops where you can get your hair cut or your face shaved. The barber is one of the institutions of the country. There is a druggist's if you are ill, or a tobacconist's if you are well. There is moreover a telegraph-office, a post-office, ticket-offices for the theatres and the railways, in fact it is a perfect *cosmos* of everything a traveller can possibly require. You can check your luggage to any place and never trouble your head about it until you arrive at your hotel at your journey's end. If you want to know what it is to travel in comfort, go to America. There you will find in regular use the luxurious Pullman cars--perfect drawing-rooms—in which you can sit before plate-glass windows and admire the passing scenery, or stroll about to prevent the effects of constraint. Passengers are always ready to chat, and there is an absence of that restraint extant in

England. Thence you can pass into a sumptuous dining-room where you can obtain an elegant repast, and when you have finished that, and had your glass of wine, you go further and find another car fitted up as a luxurious smoking-room, with easy-chairs and round tables, and where you can have Parisian coffee brought to you. Then late in the evening when you go back to the drawing-room you find it converted into a bed-room, where you can turn in and sleep through the night, if so disposed—or rather if you are able so to do—for my own experience of three or four nights was, that a sleeping car was not a sleeping car for us. However it was a novelty and a sensation. The long distances and tedious journeys have necessitated these aids to comfort in America. They would be out of place in England. Nevertheless there are some conveniences in the American cars that would be a perfect Godsend at home.

There are many of us here who have had considerable experience in railway working in England. What would you think of a splendid trunk railway being worked without any signals, and where there are no names of stations conspicuous? Where no bell rings nor whistle sounds to start the train? Where no porters look after your luggage and everyone has to look after himself? Where you need not take a ticket but may pay *en route?* "The conductor" of the train is more like the captain of a ship. He is "hail-fellow well-met" with everyone. If he asks you a question you feel bound to answer him demurely. He wears no uniform, but is generally distinguished by a magnificent diamond pin.

The railways run on the level through towns without fencing or protection of any kind, and in one town (Elizabeth, New Jersey) the two principal railways cross each other on the level in the very centre of the principal street! I cannot dwell upon these social matters because you are naturally anxious to hear about what concerns us more. I could occupy you a considerable time with details of the pleasant friends we met, the pleasant hours we enjoyed, the sumptuous river steamers we boarded, the different scenes through which we passed, and the amusing episodes that occurred.

This great country is peculiar in its organization and many may not be acquainted with its constitution. There are thirty-eight partially

independent states, nine organized territories, and two unorganized territories. Every state is practically an independent state; each making its own laws, its own taxation, and is to all intents and purposes as much an independent country as France or Germany; while the representatives of these several states meet periodically at Washington to deal with imperial laws and questions, and mould together all these separate members into one well-cemented self-governed body. The Union is certainly a marvellous solution of an intricate political problem, and if the honest and the educated classes would only take part in the government of their country the constitution of the United States would leave little to be desired. As it is, corruption and dishonesty are so rampant that they have become a source of chaff rather than of shame. The openness with which the Americans pronounce their crying sin was one of the strangest features of our visit.

The constitution of public companies in America is very different from the constitution of public companies in England. As a rule, in England we have a board of directors, presided over by a chairman and vice-chairman, having under them general managers, secretaries and executive officers, but there is a clear and broad line drawn between the administrative and the executive departments. The chairman and board of directors rarely, if ever—in fact never—interfere with the executive branch. They authorise all that is done, but for the chairman or board to take the absolute executive charge is a thing not known in England. In America the president of a company corresponds with our chairman, but he is not only chairman of the board of directors but also the general manager of the concern; he is in fact both the administrative and the executive head of the departments; and the vice-president in the same way takes under his charge distinct administrative business. The vice-presidents have various branches under their direct control, and are to all intents and purposes paid executive officers.

The telegraph system of the United States is a very large one. The absolute and correct statistics of the mileage of wires, the capital embarked, the number of stations, and various facts which we were anxious to get, we could not obtain, because such an enormous number of companies had been started, and had been in

existence and had subsided, because different States and companies had different ways of keeping accounts, and because many companies found it politic to keep the information to themselves. However, we found that, altogether, in North America, there are about 300,000 miles of wires. Of these 300,000 miles the Western Union Company absorb by far the larger portion, having nearly 195,000, the Atlantic and Pacific have 36,000, the Telegraph Company of Montreal and Canada 20,000, the Dominion Telegraph Company of Canada 7,000, and other smaller companies and railways about 40,000; so that the actual length of wires in the United States and Canada is nearly 300,000 miles.

There are, altogether, 11,660 stations, and the total number of messages sent in the last twelve months was twenty-eight and a half millions. Invested in the present conduct of this large business there lies a capital of sixty million dollars, or about twelve millions sterling. How much capital has been sunk and lost is utterly unknown. At different periods of the history of the States there have been telegraph manias, just as there was a railway mania in this country in 1845, during which period, companies sprang up in every spot, and the amount of money raised, sunk, and lost, will perhaps never be known. The Western Union itself is a combination of no less than over two hundred distinct companies, that have at different times been brought into existence during these manias. In addition to these large companies there is quite a number of local companies. For instance, there is in New York a Gold and Stock Company, corresponding with the Exchange Telegraph Company in London. This does a large business in New York in supplying private wires, in communicating the variations in the price of gold and securities, and furnishing facilities that are most surprising to see. Then there is another company, the Law Telegraph Company, the offices of which are connected with the central station, by means of which lawyers can communicate with their clients, or with each other, with the greatest ease. There are domestic and district telegraph companies, which do an enormous amount of work. Then there are Railway Companies which have separate telegraph organisations of their own, and there is a large military telegraph system. In many parts of the States where the population is

sparse, especially on frontier lines and where the Indians are troublesome, the Government have constructed for their own purposes lines of military telegraphs. There are about 2,500 miles of such military telegraphs in the United States, and these not only serve the purpose for which they were originally constructed, but, being carried round by the sea-coasts, do good service by conveying meteorological observations, which are not only a great boon to America itself but frequently prove a boon, though sometimes a nuisance, here. I say a nuisance, because when one is recovering from the effects of a fearful gale it is a terrible bore to be told that there is another to come five days afterwards. Nevertheless, these meteorological observations have, in a great many instances, proved correct, and we are beginning to pay some attention to those suggestions that come across the Atlantic.

Such is the telegraph system of the United States. In England we had on December 31st, 1877, 113,333 miles of wire and 5,328 offices belonging to the Post Office, and 21,977,084 messages were sent during the year. Probably the railway companies have 50,000 miles of wire, but statistics are wanting on this point.

I must, before going to the part which affects us Telegraph Engineers, tell you something about the commercial aspect of the question. The tariff of the United States is a very mixed affair. It is very anomalous. It is based probably upon a principle, but if so it is one of those things which is more honoured in the breach than in the observance. Now of course we all know that such a thing as a pure financial tariff scarcely exists—that is, a tariff that gives a fair return on the capital expended, or a tariff that gives a fair price for the work done. If any one were to ask you on what basis you should establish such a tariff you would say naturally it must be a tariff which is based upon a certain amount *per word*, and which shall vary with distance. The word is the unit of work done, and we all know that the cost of telegraphy increases with distance. But, owing to competition, to too great a desire to meet the assumed wants and wishes of the public, the simple tariff, varied by distance, has been spoilt, first by the addition of that "old man of the sea" in the way of free addresses; secondly, by giving too many words as a minimum, and by the too extensive

adoption of the uniform "penny postal" system. Now in the States the tariff is based upon ten words of text, the address of the receiver and the signature only of the sender being sent free. There is a practice there which does not exist here—which is, that a person need not pay for a message at the time. It is paid on delivery. The message is called a *collect* message. It is not much liked there by the companies, but is one of those bids for public favour which has crept in during the competition which existed between the companies. The tariff itself is divided into four distinct branches. First, there is the *local* tariff for messages in large towns or the area comprised between contiguous towns, viz., for any place within 25 miles, 25 cents, or one shilling, and for any place between 25 and 50 miles, 50 cents, or two shillings. The second tariff is very puzzling to understand thoroughly. It is called the "square rate." The whole of the North American continent is laid out in squares, each square having a side of 50 miles, and the tariff operates between any office in any one square and any office in any other square. For instance, the tariff between Chicago and New Orleans would be the tariff determined by the distance between the centre of the square in which Chicago is situated and the centre of the square in which New Orleans is situated. Again, taking St. Louis and Boston, the tariff would be determined by the distance between the centre of the square here [pointing on the map] and the centre of the square there [pointing], and so throughout the whole country. The tariff is divided in this way: For a distance of 100 miles, 40 cents; for 200 miles, 50 cents; for 400 miles, 75 cents; for 600 miles 1 dollar; for 800 miles 1·25 dollar; for 1,000 miles 1·5 dollar. That is in itself a very simple thing when you understand it, and if it were uniformly carried out; but, when you get to distances beyond 1,000 miles, then another tariff comes in, which is called the "*State rate;*" that is, between State and State over distances of 1,000 miles, where the tariff is based upon no principle, but is arbitrary, and varies from 2 to 3 dollars for 10 words. Besides that they have *special rates*. In some cases these square and State rates bear unequally, and, where competition has sprung up between places, special rates have been made. When we were there, there was a powerful and active competition between the Atlantic and

Pacific Company and the Western Union Company, the result being the introduction of a uniform special rate of 25 cents, or 1*s.*, for 10 words, and in the New England States the maximum charge for 10 words was 30 cents. They have introduced the very excellent plan of charging for extra words beyond 10 words at per word. Our system in England is to make all extra charges rise by increments of 5 words. The American system is decidedly superior. The average tolls per message taken by the Western Union is 43·6 cents, or 1*s.* 9¾*d.*; in England it is 1*s.* 2*d.* Practically the cost of telegraphing in England is cheaper than in America; on the other hand we must remember the average distances in America are considerably greater than in England, but, neglecting mileage distances, a message of 20 words which would cost 1*s.* 10½*d.* in America would cost only 1*s.* in England. There is, however, no fair comparison to be drawn between the telegraph systems of England and America. For instance the length of our messages is different. The average of English messages is 30 words; the average of American messages is 23 words; while the average distances of messages in America is several times that which messages travel in England. It is therefore difficult to draw comparisons between one and the other, and I do not intend to do it.

There is another system in vogue in America which we have not in England. They have a system of cheap *deferred* messages. A person can go to an office and send a message of ten words for half-rates on condition that it is sent at the convenience of the Telegraph Company and is not delivered till the next morning. Between towns where the post is slow, for instance, between New Orleans and New York, which is a two days' post, these deferred messages are very much patronised by the public. In fact, at New Orleans, of the total telegraph traffic 42 per cent. is done in deferred messages; but as we come nearer to New York, where the post is more rapid, and where letters posted at night are delivered the next morning, these deferred messages drop down to a very low figure. At Baltimore they are only 6 per cent. of the whole traffic. However, 13 per cent. of the whole traffic of the Western Union Company is made up of these deferred messages.

With regard to the Press, the arrangements in America are very

similar to the press arrangements here. News is collected by press associations. It is carried by telegraph companies at lower rates, just the same as here. In America the rate for the carriage of these messages varies from one cent to ten cents per word. The lowest rate is one cent per word for every 500 miles. Additional copies are sent to the newspapers in the same town for half-rates, so that if a press association sent a message of 100 words to a newspaper in any particular place in the States it would cost at the minimum rate four shillings, and if an extra copy were wanted in the same town it would cost two shillings. In England the same service would be performed for one shilling and two pence respectively.

Now we all know that the rates in England are very low, indeed too low, and while the lowest rate in America is one cent per word the uniform rate throughout the United Kingdom is a quarter of a cent per word. The result is that a considerably greater amount of press work is done in England than in America. In fact Mr. Fischer and I were greatly disappointed at the small amount of the press work done there. Now in England we often send on a busy night 500,000 words from the central station alone. There are also twenty-two special circuits worked in the newspaper offices, despatching about 10,000 words each. Thus on such nights our wires transmit about 700,000 words of news, and owing to so many stations working simultaneously on the same wires we deliver over two millions of words to the different newspapers. You may therefore readily understand we were not much struck with the amount of press work, about one-tenth, done in America.

There is one other class of business done there, viz., Government messages. Some years ago Congress passed a law by which every telegraph company subscribing to its terms was brought under the imperial law of the country on condition that they undertook to transmit Government messages on terms to be settled by the Postmaster-General. The result is they are now obliged to transmit messages for the Government at the rate of one cent per word for every 500 miles, and they do a pretty large business at that rate.

Free messages form a large portion of the business in America,

and last year 712,000 messages were so sent. These free messages are often given for rent, and, as we used to do in England, they are given to railway companies for something received in return; *now*, unfortunately, we give it them for nothing received in return.

The same arrangements for repetition and insurance exist in America as used to exist here, and they carry on a large money transfer system which brings them in an annual revenue of 92,364 dollars. Money is sent for one per cent. commission; and 2,464,172 dollars were so sent last year by the Western Union Company. The largest sum sent is limited to 1,000 dollars, and that only to a few stations. The average amount transmitted is a small one.

Porterage is cheap, and messages are delivered within half a mile free.

I will now say two or three words about the staff. In America the telegraph service is quite a favourite service. The demand there is very great, and we always know that when the demand is upheld there is no falling off in the supply. It is looked upon with such favour and is so much thought of that it is quite an exception to find anybody who has not some knowledge of telegraphy. At Harvard College—our Oxford and Cambridge combined—the undergraduates there have in their rooms their own telegraphs, which they put up themselves. They form themselves into companies, and it is one of the honours of the University to be associated with the leading companies of telegraphists. The people on the railways, station agents and superintendents, are also employed to a large extent on telegraph work, and nearly every station-master has been at one time an operator. The pay is good. The average pay of the Western Union operators is 192*l.* per annum, while in England the average pay is only 80*l.* Operators all over the country take great pride in their work. They look upon telegraphy as an art to be cultivated. You can hardly take up a professional newspaper without finding operators referred to by name, in terms more or less eulogistic of their abilities, such as "Mr. So-and-So is an excellent operator," "Mr. So-and-So has made very pretty copies," "Mr. So-and-So never 'breaks,'" "Mr. So-and-So did so many messages in a given time."

Here is such an extract: "Greensburg, Pa. The late Jesse Mills and Mr. Kettles, of Boston, sent and received 518 messages in nine hours. Messrs. W. G. Jones of Philadelphia, De Graw and McCarty of Washington, Phillips, Boileau, Taltavall, Baldwin, Moreland, Catlin, Bennet, and other New York men, have all made time nearer to fifty words per minute than to forty. We believe that Mr. E. C. Boileau, if not actually a faster, is a *faster and better* sender than any operator in this country; but if you know of anyone who has done better, and the case is well authenticated, we shall be glad to make it a matter of record." In fact great display is thus made of the abilities of the operators, who have thus established a kind of gazette of their doings in their art. The result is, these men take great pride in their profession, and they consequently acquire great skill in it. We have unquestionably excellent operators in England, some of them quite equal to the best Americans, but they are the exception not the rule. Taking their scientific attainments as a class, I do not think they are superior, or even equal, to the attainments of our best class of operators in England, for the simple reason that in England we have submarine cables and long underground lines involving the most abstruse laws of electricity, and we have more complicated and higher-classed apparatus. In America you see every man operating with his right hand and timing with his left, and *vice versâ*, and one was known to have sent a message with one hand while he received another message at the same time with the other. We are much inclined to discredit alleged displays of skill, because we are not able to do the same ourselves. We laugh sometimes at things we do not understand, and, if we are shown a complicated piece of apparatus which we cannot follow, we smile at it as an amusing and unnecessary thing. So it is with these displays of skill on the other side. I was myself as sceptical as anyone could be on these reported matters, but having seen some of that kind of thing done I came back quite a converted individual. One great incentive to progress is, that in America ability always secures its own reward. It is a saying that a French soldier always carries a marshal's baton in his knapsack, and so it can be said of an operator in the United States—he can rise up to

any position in the telegraph system of that country, and it is a remarkable thing that amongst all the men I met on the other side connected with telegraphy there was not one who had not been an operator and who was not proud of acknowledging that he had been one. There are male and female operators. Females are not employed in telegraphy in America to quite so large an extent as in England, but still they are engaged to a very considerable extent. Their skill was as marked as that of the male operators, and they displayed an excellent knowledge of the technical branch of their business. Special engineering assistants are not known in the United States. No special means are taken to train up operators in technical and scientific attainments. "Self-help" is the motto of their education. But telegraphic periodical literature flourishes. Manuals and text-books abound. Electrical Societies for the mutual interchange of ideas and the imparting of knowledge are springing up, and the spirit of the age appears to be there—as it is here—progress.

There is one curious departure from our practice, and that is in their alphabet. It differs considerably from ours. I have not given here all the letters in which they differ from us, but I show some particular letters. They have what are called space letters.

	ENGLISH.	AMERICAN.
C	— - — -	- - -
L	- — - -	——
O	— — —	- -
R	- — -	- - -
Y	— - — —	- - - -
Z	— — - -	- - - -
&		- - - -

Take, for instance, "C." It is made up of two dots, space, and one dot, the space being equivalent to two dots. "L" again, dash—

equivalent to six dots. "O" is made of two dots, separated by a space. "R" is made up of one dot, space, and two dots. "Y," space and two dots. This is the alphabet first formed by Morse, but there is no question, and they admit it freely, although they believe the space letter system is quicker than ours, that it is nevertheless the cause of an enormous number of errors; and there are errors, or "bulls," as they are called, there, as there are errors here. I think, however, that their errors are principally due to these space letters, and some of them are strikingly curious. I will instance one to show you:—A lady received this message—"Mr. Sage has caved in, and is satisfied," whereas the proper message was—"Message received, and is satisfactory." Did time permit I could give heaps of such amusing "bulls."

Now, I come to the department more immediately affecting us—that is, the engineering details of the American system, and in this department immense variation of practice has stepped in, due principally to the difference of climate, to the great distances which separate the different places, and also to the different and more difficult means of locomotion. The leading features of American telegraphy are extreme simplicity, great uniformity in the character of their stores, and in their mode of carrying out their works.

Now, with regard to construction. The same questions arose there as have arisen here, as between the relative merits of roads and railways, and, like the practice in England, the first telegraph companies made a start for the railways, and secured them as fast as they could. The result was that competing companies were forced to the roads, and we now find the country covered with telegraphs upon the railways and upon the roads. The cost of construction of telegraphs in America does not differ very much from the cost here. The average cost of a one-wire line varies from 100 to 150 dollars per mile, according to circumstances. That is not much more than we pay here. They have not much underground wire; in fact, the only piece of underground wire I saw is in the City of New York, and that is from the Central Station to the River Hudson, where two lines, 400 yards long, of 3-inch pipes, with 30 wires in each, are laid down. They go through their towns right through the main streets, and nothing is more striking to an Englishman's

eyes than to go to New York and there see all the principal thoroughfares crowded with telegraph poles, disfiguring the streets right and left, and covering the sky-line with disfiguring clouds of wires. There are no less than ten distinct systems passing through the streets of New York, and in some streets no less than four distinct lines of poles crossing each other in every way. As the young gentlemen of America are as fond of kites as those in England, the result is that in Cincinnati I am afraid to tell you how many strings I saw fixed to one wire. It was an experience worth gaining to have learnt the possibility of such a state of things existing without causing trouble.

These pole-lines are some of them very fine indeed. They do not attach the wires to chimneys and roofs, but prefer carrying them through on poles. Going through New York, I saw two poles 96 feet high, with over 100 wires on them. In Philadelphia I saw poles 90 feet high; in Chicago 75 feet high; but all these lines were put up at tremendous cost, and I cannot help thinking that the opposition in America to underground wires is one of prejudice. I am certain that in New York and Philadelphia underground wires would be infinitely preferable, and more economical than those very massive poles. New York is on an island, and is separated from New Jersey by the Hudson River on the one side and from Brooklyn by the East River on the other side. These rivers are crossed by submarine cables of 7 wires made up to a weight of 8 tons per mile, and these are taken up for repairs and laid down again with great celerity. Owing to the nature of the bottom of the rivers, ships drag their anchors very much, and it is a frequent thing for the cable to be hooked five or six times a day. The result is that the Western Union Company is obliged to keep a tug always ready, with steam up, to help ships to clear their anchors and put all right again. But sometimes the cable gets damaged. I see here to-night some submarine engineers, and I have myself had some experience in repairing submarine cables, but if anybody had told me twelve months ago what I now tell you I should have listened and not believed. This will doubtless be the case with many of you, but in my case I had a witness and companion with me, who took the time with me. This steamer sailed

up the river Hudson on one occasion whilst we were there, hooked the cable, and picked up a mile and a-quarter of it in 20 fathoms water in 40 minutes, and then laid it down again on another route in 6 minutes. A submarine cable which in England would take a week to pick up and repair, and another week to lay down again, was picked up in 40 minutes and laid down in 6, the whole expedition not occupying over 4 hours.

Now, with regard to their overland lines, they are as sound and good as anything we can produce in England. The poles are of an average height of 25 feet, and 6 inches in diameter at the top, principally of cedar, white or red, and they cost from 50 cents to 1 dollar a-piece. The price varies very much; they last under ordinary circumstances from 12 to 20 years. In many places they are of chestnut. The chestnut in America grows in a different way from what it does here. Our chestnut grows irregularly, but in America it is straight, and forms very excellent telegraph poles. In some parts they are of locust, a tree not unlike our laburnum; the flowers are however white, and it is a very pretty tree. In some places they use cyprus poles, and on the Pacific coast they use pine obtained from the primitive forests, a cheap timber, which lasts a long time. In England unprepared larch telegraph poles last 7 or 8 years, but in America their average life is about 15 years; the climate is so dry that it conduces to the preservation of timber by thoroughly seasoning it *in situ*. The result is that no preservative process is used, and I saw no instance of iron poles being used. The arms are of white pine, and of the uniform scantling of 4″ × 5″. Two wires require 3 feet arms; four wires 5 feet 6 arms; six wires 7 feet 6 arms. Where one wire is used they have oak and locust brackets of this kind (exhibiting), and sometimes they have the same insulators placed on the top of the pole by means of pins.

The wire used is sometimes galvanised and sometimes ungalvanised. It was a source of surprise that in such an enlightened country they could put up wire ungalvanised, but those that had done it had good reason for doing so, at least they were able to adduce satisfactory reasons to themselves, the chief reason being economy, the chief culprits being the poorer companies, who were running

the opposition. The great company—the Western Union—never thought of putting up *ungalvanised* wire. The question was raised while we were there as to the relative merits of the two, but the evidence from all parts was so much in favour of galvanised wire that I doubt very much whether any one in America would have the temerity to propose again the use of ungalvanised wire. The gauge of wire is No. 9 for short lines, No. 8 for medium lines; but No. 6 is used more generally, because practically their lengths are so great.

To America the credit is due of being first to introduce into iron wire the test for conductivity. That is rather a disgrace to us. We did not do it here. They have done so, and we followed their example; and I believe the example will be generally followed all over the world. The result has been much chagrin to wire manufacturers but much improvement to the working of telegraph lines.

Now, in America, they started what is called the *ohm-mile.* They specified that the quality of material of which the wire was composed should give an ohm-mile equal to 5,500 lbs.—that is, a mile of iron wire weighing 5,500 lbs. should give one ohm resistance. This would give fourteen ohms per mile to No. 8 wire. The result has been that they have succeeded in getting a wire which gives an ohm-mile weighing 4,884 lbs. The best we have hitherto got in England has been about 4,900 lbs., so that besides introducing that conductivity test they have succeeded in producing wire superior to what we obtain in England. There is a form of wire used in America called compound wire, consisting of a steel core surrounded with a tape of copper drawn through a tin bath. At least I do not know for certain, but I believe they used the tin bath. We were told that this wire had not succeeded, owing to the water getting in through the parting of the copper strip from the wire; the steel rusted, and the whole thing "burst up." Messrs. Wallace, of Ausonia, Connecticut, are producing a wire coated with copper by an electrolitic process. In England Messrs. Siemens are also improving that process, so that sooner or later we may succeed in getting a perfectly covered wire. No doubt it is a matter of great consideration to get a wire of the strength of No. 8, with the lightness of No. 16, and with the conductivity of No. 4.

The wires in America are pulled up rather loosely. Judging by the eye they are not pulled up with a greater strain than 200 pounds, in England our strain is about 300 pounds. We are obliged to do that, because our wires are carried closer to one another than they are in America, and we regulate our wires better. In America the arms are fixed 22 inches apart, and the arms being long the wires are separated by considerable distances, and in these days of fast speed telegraphy I think that that is a great advantage. We are very fond here of the Britannia joint, the joint introduced by Mr. Latimer Clark. We look upon that as the joint of all joints. The joint is stronger than the solid wire itself, and it gives no trouble. But they do not like it in America. Their joint is a mixture of the German twisted joint and somebody else's joint. It is a twisted joint, the centre of it being a long spiral, while each end is turned round three or four times. It is then dipped into a bath of solder, when it forms a good joint both electrically and mechanically, but not quite so good as the Britannia joint, for I have known cases of the wire breaking at the joint. The practice of soldering joints is now being generally introduced into America. We have always soldered our joints in England. Bad joints we never expect because we always solder them; but in America they have hitherto neglected this to a large extent, and in Canada in this year of grace 1878 they still do not solder their joints. The result of soldering has been this,—that, whereas a certain circuit with unsoldered joints gave a resistance of 23,500 ohms, as soon as the joints were soldered it gave a resistance of only 1417 ohms. I am happy to say that they are doing this in America now as fast as they can, and in course of time the whole system of the Western Union will be provided with soldered joints.

Now, in this large system of wires of 200,000 miles of the Western Union they only use one kind of insulator. That insulator is this annealed green glass insulator that is familiar to many of us (exhibiting). We used it largely many years ago, but we found that these insulators burst up from a cause that is absent in this insulator. We used to fix the iron bolts in our insulator with cement. At first we used sulphur, and the sulphur acting upon the iron formed a sulphuret of iron which expanded and burst the insulator.

The result was, walking on the line you often heard them crack off like pistols. They have got over the difficulty in America by dispensing with the use of cement altogether, the insulator being screwed on to a wooden pin. As an insulator *per se* that is a very sorry affair. We could not work a line from London to Birmingham with such an insulator as that, but in America they have a bright pure atmosphere; fogs are scarcely known; those great aqueous clouds that envelope our country from end to end are never known, and my impression is that for several months out of the year they could almost work from New York to Chicago without insulators at all. There are some lines on the New England coast where the climate is very similar to that of England, but they have not got the aqueous clouds, as I term them, coming from the sea on to the land. The coast of New England skirts the ocean, and there are winds that blow from the sea to the land, but those winds come from a cold climate to a warm one. They pass over a polar current and come to a warm land. On the contrary, we have also winds coming from the sea to the land, but they come from a warm current to a cold one, and the result is they come charged to saturation with saline and other matters, and they coat all the insulators in their path with a conducting mass. The result is, that, however perfect the insulators, there are some lines in England where the insulation frequently breaks down. There are other insulators used in America. There is one called the Kenosha, made of baked wood steeped in some insulating compound. There is also another kind of insulator much used there on railway lines, and we have heard a good deal of it in England. It is one which has carried off a great many prizes at exhibitions, and I am pleased to say we have the presence here this evening of its inventor, Mr. David Brooks, from Philadelphia. It is used over many thousand miles of wire in the United States, and as an insulator *per se* no doubt it is a very good one.

There are other classes of insulators adapted for parts where the wire runs through forests, where falling trees are a great source of trouble. In such places the wire is threaded through, as we originally did in England, and even now do on the South Eastern Railway. They do not bind the wire to the insulator with small wire,

as we do, but they tie it by one turn of No. 8 or 9 wire. They do not use shackles, they are wiser in their generation. We have too largely used shackles. Staying and strutting, "bracing" they call it, is carried out only to a very small extent. Throughout my trip I did not see a dozen stays or struts. They strengthen their lines by freely using poles—40 to the mile is the usual number.

Again, the practice of earth-wires is not carried out to a large extent. The earth-wire is regarded as our safety-valve in England. It was originally introduced with the idea of protecting the poles from lightning; but it was found that an earth-wire run up a pole not only protected the pole from lightning, but carried away to earth extraneous currents that leak from one wire to another. The result was that where earth-wires were used we suffered in no way from weather contact, and we have since universally established the system of earth-wiring our poles. They have been introduced in America, but only to protect the poles from lightning. At some seasons their thunderstorms are very intense; in fact, the severity of our storms is nothing to theirs, and when a stroke of lightning does strike a line of poles they make a sorry figure of that line: 100 or 200 poles may at once be destroyed by a single flash; but wherever earth-wires have been erected this effect of lightning has entirely disappeared.

The leadings-in are extremely simple, economical, and effective. Their open-wire system enables them to effect this. Covered wire is very seldom used except inside the offices. The wires are invariably led to plate lightning-protectors, often placed in a cupola at the top of the offices.

It is rather a relief to find something to criticise unfavourably. Their battery system is indifferent. They use that extravagant form of battery—the Gravity—introduced in this country many years ago by Mr. Cromwell Varley, and speedily abandoned. Based on a fallacy, it leads "to wasteful and ridiculous excess." It used to quite irritate me to find this form of battery so very largely used, and on the closed circuit principle too. Its maintenance cost them on the average 5*s.* 5*d.* per cell per annum. Our cells—the simple Daniell, with porous earthenware cells—cost less than 1*s.* Having previously used the Groves' cell, this form to them has

been a great improvement and a great economy, but there is no doubt that a lesson or two from England in this department will effect a still greater improvement and a greater economy. We recently had the pleasure of a visit from two of their ablest electricians, Messrs. Gerritt Smith and Hamilton, and I have little doubt that these gentlemen will effect a reformation in this department.

The telegraph lines in America are maintained in a very high degree of excellence. The criterion of the proper maintenance of a line is, first, its freedom from breaksdown; and secondly, the rapidity with which breaksdown, when they occur, are repaired. There is not much to say in favour of our country against theirs. Taking the mileage, we suffer from as many faults as they do, and they repair them in about the same time. But there is one practice in which they differ from ourselves, that is, that the responsibility of removing office faults is thrown upon the operating staff. Every manager of an office is responsible for the condition of the apparatus in his office. There are no such officers there as inspectors. There is nobody between the operator and the superintendent. The repairer or lineman is a man who receives a salary of about £150, and is paid about as well as an inspector here. Indeed, he is to all intents and purposes an inspector.

The testing there is done entirely by the operators. There is a chief operator in each office, whose duty it is to be responsible for the condition of the batteries and apparatus, and to test the lines. One feature in every office is the total absence of galvanometers. If a fault comes on the clerk will test with his fingers. The climate is so dry, and the insulation so perfect, that they can, by receiving the return shock, with great accuracy localise the distance of a fault. I succeeded myself, by a little practice, in localising a fault within ten miles by the strength of the discharge received on my fingers. They now keep up their lines in a high state of conductivity, as I have previously remarked. The general system of working here is the closed circuit-system. This system has a great many advantages; in fact, the leading principle in telegraphy is that of simplicity and uniformity, and this is aided very much by the closed circuit system of working. It compels all the batteries

to be concentrated at one spot. It simplifies the adjustment of the apparatus to the varying currents on the line; it enables many more stations to be worked on the same wire. I saw a circuit 457 miles long working well, with 57 stations upon it. It applies a constant test to a circuit, and it has many points which commend themselves to our notice, and we shall to a certain extent avail ourselves more largely of the principle. On the other hand, open circuit working where the battery is lying idle at times is the principle adopted in this country. A circuit previously worked with 250 cells on the closed circuit system was more efficient with 75 cells on the open circuit system. In long circuits the open circuit is unquestionably the right system, and I have no doubt that by the introduction of the open circuit working they will be able not only considerably to reduce their battery power but to work better.

With regard to the forms of apparatus, telegraphy in this respect in America is reduced to simplicity and uniformity. The instrument found in every office, and that which is the basis of their system, is this simple little "sounder." I have brought one or two specimens of these sounders for your inspection to-night; and wherever you go you find nothing but a simple key—here is one of these keys [exhibiting]—a relay and a sounder. These are all that are required to fit up an office in America; the battery, working on the closed system, is at the terminal station. Here we have telegraphy reduced to the greatest point of simplicity, and the cost of maintenance minimised to its lowest limit. By the use of the sounder for the transmission of messages you increase the capacity of the wires for the transmission of messages and secure greater accuracy in the despatch of business. Many of you are aware of my opinions as to the advantages of "sound" working, and if I have learnt nothing else from my trip I have learnt the immense advantage of that mode of working. The keys are simple. The relays, which are nonpolarized, equally so. The distinguishing features of their relays is their low resistance, (150 ohms,) and the smallness of their coils. They have thus by practice arrived at the same conclusion as we in England, that faster and better working is attained by reducing the size of the cores of relays and their

resistance. Theory, from neglecting conditions, has been very far out in indicating the proper resistance of relays.

Working as they do to great distances, they have carried out "repeater" working to a large extent. Direct working is rarely carried on to a greater distance than 600 miles; beyond this, repeaters are used sometimes two and sometimes three for their longest distances. Between New York and New Orleans, for instance, 1,450 miles, there are repeaters at Washington and Augusta. There is scarcely an office of any size which has not several of these repeaters. Washington, Baltimore, Buffalo, and Boston have five or six repeaters each, which are used regularly and irregularly. When the weather becomes bad, and troubles arise, they fly to these repeaters. One of the most striking features of the repeater is this—that the introduction of two or three repeaters on the circuit from New York to the long distances to Chicago, New Orleans, St. Louis, &c., does not interfere with the rate of working, and the messages come off as quick as the clerk could read them. That, I may say, is contrary to our experience in Europe. The introduction of translators on long circuits worked by key has been to lower the rate of working, and the greater the distance the slower was the rate of working.

Duplex working has met with great advance in America. It was, indeed, resuscitated there. It remained dormant for many years, and then, on its introduction by Stearns, who overcame the difficulties of working by the application of the condenser, it received a fillip which has forced its employment over the whole world. In America it is of course used to a very large extent. It was also used by the opposition company, the Atlantic and Pacific, but as they had not the right to use the patent for condensers they have been obliged to dispense with them. They used a system known as D'Infreville's, but which is the system introduced here by Mr. G. K. Winter. It is an extremely simple and rapid mode of duplex working. There is also another duplex system by Haskins which is in use by the North Western Telegraph Company. Duplex telegraphy led to diplex. By the former, two messages could be sent on the same wire in *opposite* directions; by the diplex your two messages can be sent in the *same* direction on

the same wire; whilst by the quadruplex system you can send two messages in each direction at the same time; and, not to weary you by other "plexes," every other style is called multiplex. I have seen all these systems worked. It would take a whole evening to attempt to describe the quadruplex working. It has been before the Society before. It may shortly be stated to depend upon these principles—that you have in the first place a duplex system worked with reversed currents; secondly, that you have the duplex system worked with increment and decrement of the current, and you have those two working together. The two things work side by side with perfect success. The quadruplex system has been established between London and Liverpool, and nothing could be more successful. Multiplex working is this—the system introduced by Mr. Gray at Chicago was based upon the same idea as the patent taken out by Mr. Cromwell Varley in 1870. The principle of the two is identical. Between Chicago and Dubuque there is a circuit providing for seventeen intermediate stations. These work as ordinary Morse sounder circuits, and are now working as such; but between the two terminal stations there is on the same wire a telephonic circuit, or rather there is a circuit worked by the rapid vibrations of another battery, which, being simple vibrations, superpose themselves on the working currents. They are received at each end by telephonic apparatus, and Chicago can work to Dubuque without interfering with the messages at the other stations, which are worked by means of the ordinary sounder. This has been tried on other circuits, but when I was there it had not arrived at such a condition of perfection as would justify the recommendation of its being tried here. I have heard since that it has been found extremely practicable. While 225 messages were exchanged between the various stations on the circuit by means of the sounders, 300 messages were exchanged on the same wire between Chicago and Dubuque. I have no doubt one fine day we shall find some one coming over here with apparatus for us to try.

Type printers! The type printer also originally sprang from America. Our friend Professor Hughes tried years ago to introduce type-printers into America, but did not find our friends there

so ready to accept his apparatus. He came to England and established the Hughes' apparatus in Europe. In America this same apparatus which has since been perfected by Professor Hughes was worked, under the able guidance of Mr. Phelps, on the Western Union system, and he has converted it into another printer, which however in its main principle is the same as Professor Hughes's, though differing from it in detail. He calls it an electromotor machine, because it is driven by an electro-magnetic machine, but in other respects it is the same in principle as the Hughes. It is not used to a large extent by the Western Union Company, but a modification of it is employed to a considerable extent for private wire working. There is another form of type printer by Mr. Gray, and one by Mr. Edison, which are used for the transmission of commercial news and prices, and for private wires by the local telegraph companies.

Messrs. Welsh and Anders, of Boston, have produced a magneto-electric type printer, which is very highly spoken of, and is largely used for private lines. Another form of instrument I saw was Edison's automatic apparatus, which has been extolled to a large extent as producing wonderful results, but it is not an instrument that works well except on short circuits. It is used by the Atlantic and Pacific Company, and, though it was worked to its utmost limit for our inspection, we did not get more than 200 words per minute out of it. It depends upon an electro-magnetic shunt; but sooner or later I have no doubt it will receive some extension.

Since we were there we have had introduced to us that very wonderful instrument the telephone. I saw it when it was first introduced; since then it has received a large amount of attention in America, where a great number of instruments are in use, and it has created a sensation here which is likely to continue for some time to come.

Some of the technical terms used in America are peculiar. A poor operator is called a "plug." To stop a message is to "break." If an operator cannot call a station it is said he cannot "raise NY." Contact between wire and wire is called "cross-fire." Connecting one line across to another is "flipping;" working off business is "rushing;" and when a message from an inter-

mediate station takes off a message at the same time that it is being sent to the terminal the wayside station takes a "drop copy." If there is a great pressure of messages "biz is piled up." If a wire works badly it is "a rocky wire." If the current fails the battery "loses grip." When a wire breaks down it is "busted;" if it is faulty it is "in trouble." An intermittent contact is "a swinging cross." Forms are "blanks." There are other terms very peculiar, but to us not very expressive, because we are not acquainted with them.

There are various peculiar accidents which telegraph lines in America suffer from. One is the falling of trees on forest lines in winter and another is lightning in summer. Forest fires are a great source of trouble, and they suffer more than we do from snow and sleet storms. However their electrical condition is so perfect and the climate so favourable to the working of wires that these exceptional cases of break-downs do not trouble them so much as they would trouble us, and I was surprised to find that they do not suffer from lightning so much as we do here. I attribute that to the fact that our system is more concentrated, and to the little thunderstorms we have rarely occurring without affecting our wires and damaging our instruments; but in that great country they may have fifty storms and not one pass over the telegraph wires.

I should have liked to have said much about the construction of their workshops. One distinguishing feature is the almost total absence of mechanics; the practical skill of the operators renders it unnecessary to keep mechanics.

They have in America a large telegraph business in ship-signalling. The arrival of mails is looked for with great interest, and they have carried the signalling to a high pitch.

I have alluded to the military telegraph stations. They have 145 stations all over the country, and, as I have mentioned, these stations are used to a large extent for meteorological purposes. They watch the rise and fall of rivers, and they give notice to farmers of the approach of thunder and wind storms; in fact the transmission of these observations is doing a vast amount of good.

Then there is time-signalling there. The time at Washington is sent to I don't know how many different places.

I am now going to tell you something which I am sure you will not believe, because the results appears so incredible. There are fire alarm apparatuses all over the towns. Wood being used to a large extent in building, when a fire occurs, unless the engines are quickly on the spot, the building soon comes to grief. The conditions of building are so different to ours, that, whereas we should have difficulty in setting fire to a house, a slight spark might cause a disastrous conflagration in America, and it is this which renders necessary the wonderfully rapid system of fire alarms that has been established there. The consequence is that the most elaborate and beautiful system of fire-signalling is established in every town. Wherever a fire occurs, a person pulls a handle which rings a bell at the fire stations, indicating the spot; this is immediately transmitted to the other fire stations, and the engines arrive and the fire is mastered before it has got ahead. I have not found anyone who will believe what I saw. We inspected one of the fire signalling stations in New York, and an alarm was raised. The gong sounded, the men turned out, the horses were unhitched and rushed to their places at the engine, and all was ready to start in *eight seconds !* We took three observations of these exploits; the first was eight seconds, the second seven seconds, and the third nine seconds, so that on an average of eight seconds this operation was performed. It is recorded that in one instance there was a fire one-third of a mile from the station. The alarm was given, the horses were released and hitched up, and the engine was off and was playing on the fire in one minute forty seconds.

In Chicago this system is carried further. Not only does the sounding of the alarm release the horses ready for putting to on the engine, but in a room above there is a long sleeping place for twelve men. At the foot of the driver's bed there is a trap-door. When the alarm is sounded every horse is unhitched, and runs to its place: the bed-clothes are whipped off every man: the trap-door falls, and the driver slides down into his place! I saw that done: I did not take the time, but I believe it was all done in six seconds. They even go further than that. They do not require to see smoke and flame issuing from a building, but they have automatic alarms there, which are set in motion by the warmth of the

atmosphere, which causes contact to be made, and communicates the alarm to the fire station without intervention on the part of anybody.

I must say one word with regard to domestic telegraphs. They carry their telegraph system to a pitch of perfection that we cannot understand. There is scarcely a private house in New York of any pretensions, or indeed in any town of over 20,000 inhabitants, which has not its call-box in the hall or in the office for the transmission of orders. Supposing you want a messenger to fetch you tobacco, or a carriage to take you to the theatre, or supposing you are ill and want a doctor, or are attacked and want the police, all you have to do is to put an index to "doctor," or "messenger," or "police" and start a handle. It immediately rings a bell at the central station, which indicates the street and number of the house as well as the message. In this way the telegraph in America is made subservient to the comfort and conveniences of social life.

I pass on however to speak of the organisation of the telegraph system. It is largely characterised by great elasticity. There is a total absence of that routine which is such a bugbear to our conservative system. Every man is allowed to act upon his individual responsibility, and he is allowed great latitude in carrying out his business. The result is, the service there is a genuine service, and you never find there a man discharging his duties in a perfunctory manner. Every man knows that his success in life depends upon his own exertions—he does his work with a will.

It has been said of us that we stand with our hands in our pockets waiting for inventions from America. That is not true. We do a good many good things ourselves, but it is not our fashion to blazon them forth. One reason however why inventors come to England is this—in America invention is a profession, men are paid there to invent. The Western Union have in their employ one of the cleverest practical electricians of the age—Mr. Edison. He is furnished with a magnificent laboratory, his only duty is to invent, and if he did not invent he might lose his employment sooner or later. More than that, people are encouraged to invent. A man shadows forth a thing—if it is pro-

mising, every thing is put at his disposal and all is done to encourage him. Every new thing is freely published, and the patent laws are sound and within the reach of all. In England, on the contrary, an inventor is looked upon with horror—as something to be avoided, and the patent laws are execrable.

I do not draw comparisons between the systems of America and England. The fact is, neither system can be said to be in advance of the other. We have all of us worked in different grooves, but we have all had the same goal in view, and the same results have been obtained. We have all striven with might and main to acquire first of all rapidity in the transmission of messages; secondly, to increase the capacity of wires for the transmission of messages; and thirdly, to improve facilities to the public. In America the increased capacity of wires has been obtained by improving the "sounder" and adding all the wonderful advantages of multiplex telegraphy. We in England have had to do the same thing by perfecting the automatic system of Bain and Wheatstone, and we have been able to do some good things in England with the automatic process. Facilities to the public have been increased, but not, as in America, with good financial results. If the Postal Telegraph Service had been conducted upon strictly financial principles perhaps some hundreds of our villages would never have had the advantages of the telegraph, they would have been left out in the cold. In America telegraphs are made to pay, and no office is opened that does not pay.

There are no doubt many "wrinkles" to be derived from what we saw done in America. We have recently been visited by two distinguished Americans, who have no doubt gained some "wrinkles" from us. Mr. Fischer and I have come back with "wrinkles" gained from them, and I am quite sure that mutual benefit must ensue from mutual intercourse between two such countries as England and America. (Loud applause.)

SIR CHARLES BRIGHT rose and said: I am quite sure I express the feeling of those present when I propose a very cordial vote of thanks for the interesting, graphic, and eloquent address which Mr. Preece has favoured us with. I have been in America, and have seen something of the telegraph system there, but I can add

nothing to what has already been said. If the time was not so limited I could have said a few words with regard to the social aspects. I have myself slept well in the Pullman cars, and I don't like the hotels which Mr. Preece praises. If there is any discussion on the subject at a future time I should like to say a few words with regard to the "sounder" apparatus and insulators; but I will leave that to another time, and content myself now with proposing a vote of thanks to Mr. Preece. I should like Professor Hughes to second it, if he will.

Professor HUGHES: I very cordially respond to Sir Charles Bright's suggestion that I should second the motion. I have been very much delighted at listening to the address, and I beg to acknowledge the kind remarks which the author has made with reference to what little I may have said or done myself.

The PRESIDENT: I feel as if I had hardly got breath to put this motion to the meeting. I confess Mr. Preece has taken the breath out of me, not only by the vast mass of matter no less than by the perfectly true statements he has made. (A laugh.) One is, however, happy to find that the amount of go-a-headedness which he seems to have acquired in the healthy and crisp atmosphere of America has not deserted him yet, and he is evidently in a fit condition to give us just such another lecture as he has already delivered. We had an account of the quadruplex system in this Society about a year ago, and it was then talked of as a perfectly experimental system. Mr. Preece promised to bring us detailed information with regard to the working of that system, but he has only been able to touch upon it so far as to say it is working successfully. The question of testing, which has been carried out in a remarkable way, and also that of insulators, is a very interesting one, and affords matter for separate discussion, and if we could have the opportunity of putting Mr. Preece on the rack again at a future time I am sure it would not only be to our advantage here but to the advantage of the profession generally in this country and also on the other side of the water.

The vote of thanks was unanimously accorded to Mr. Preece.

The following Candidates were then balloted for and declared to be duly elected:—

FOREIGN MEMBERS:

Don Julian Alonso-Prados.
Don Marco Antonio Bolton.
Herr A. Boomsma.
Mr. Bourne.
Don Fernando Cabrera.
M. E. G. Conchard-Vermeil.
Dr. Oscar Frölich.
Don Felix Garay.
Don Luis Lobit.
Don Francisco Mora.
Herr J. H. Nagel.
Don Antonio Lopez de Ochoa.
Don Emilio Orduña.
Don Frederico Garcia del Real.
Mr. L. J. Santman.
Don Pascual Ucelay.
Don Francisco Vazquez.
Herr H. C. Th. Van de Wall.
Mr. W. J. Wisse.
Don Juan Martin de Ybarolla.

MEMBERS:

Mr. Albert J. L. Cappel.
Mr. Charles James Sharpe.

ASSOCIATES:

Mr. S. Andrews.
Mr. Arthur H. Bateman.
Mr. Karel P. J. Stakman Bosse.
Mr. Henry V. Browne.
Mr. Edward Campbell.
Mr. Henry de Burgh Daly.
Mr. George Driver.
Mr. James Edgar.
Mr. J. P. Edwards.
Mr. Walter Emmett.
Mr. Pierre F. Feytens.
Mr. T. Ford.
Mr. St. George Lane Fox.
Mr. C. W. Hager.
Mr. Henry Haskayne.
Mr. A. H. Hiscott.
Mr. W. G. Lyster.
Mr. Alex. M. Moir.
Don Jose Ramon.
Mr. A. S. Roberts.
Mr. J. S. Smith, C.E.
Mr. Albert A. L. Straube.
Mr. George Robert Tapp.
Mr. John Thwaites.
Mr. Richard Tonking.
Mr. Magnus Volk.
Mr. F. H. Webb.
Mr. James W. Woods.

STUDENT:

Mr. G. A. Grindle.

The Meeting then adjourned.

The Sixty-fourth Ordinary General Meeting was held on Wednesday, the 27th of February, 1878, Mr. LATIMER CLARK, Past-President, in the Chair.

The SECRETARY read the following communication:

Liverpool, 16th January, 1878.

Dear Mr. Clark,

I am sorry I cannot send a paper on the Chloride of Silver Element in time for the next Meeting of the Society. My time the last week has been so fully taken up with preparations for my experiments at Torbay. On the other side you will find the result of my tests last Saturday of sixty of Mr. De La Rue's cells. This form of cell seems exceedingly promising as a standard of electromotive force, and I would suggest determining its electromotive force in absolute measure, by means of the suspended coil galvanometer you had made for the determination of your mercury cells. From comparison with your cell, the chloride of silver cell, as set up by Mr. De La Rue, has an electromotive force of 1·065 volts. The solution used contains 23 grammes of chloride of ammonia to the litre of water—but for a standard of electromotive force I would suggest pure water boiled free from air. The chloride of silver element would then have all the requisites of a standard cell, viz., all materials in construction of definite composition readily procured chemically pure and no local action.

If not too late, I should like you to postpone bringing this subject before the Society until my return, when I shall be able to go more fully into it.

Mr. De La Rue has kindly promised to lend the cell marked C in my tests for exhibition to the Society.

Immediately on my return I propose determining the value of your cell, and at the same time shall investigate the chloride of silver cell further.

Hoping you will excuse this hurriedly-written note,

I remain, dear Mr. Clark,

Yours very truly,

ALEX. MUIRHEAD.

COMPARISON OF 60 OF MR. DE LA RUE'S AG. CL. CELLS.

1 Cell through Galvanometer direct = 346730 divisions.

Tray 1.	Tray 2.	Tray 3.
Divisions.	Divisions.	Divisions.
C = 0	175	+ 310
45	250	120
+ 45	100	140
180	290	90
130	270	60
210	110	+ 250
0	220	0
170	130	+ 215
170	220	100
165	260	430
130	200	120
190	110	380
120	280	150
50	260	280
20	250	+ 70
310	250	+ 20
250	210	75
175	130	10
380	230	
130	150	
180		
270		

The CHAIRMAN: I have a few remarks to make on this subject, which I will read.

The letter which has just been read was written at my request by Dr. Muirhead, who had desired to call attention to the great uniformity of the electromotive force of the sixty cells of Dr. Warren De La Rue's chloride of silver battery, which he had tested at that gentleman's suggestion. Dr. Muirhead has been compelled

to go abroad, and has unfortunately not had time to complete his paper on the subject, and I will therefore supplement it by a few remarks.

These cells, as Dr. Muirhead has stated, are composed of fused chloride of silver, or "horn silver" as it is called, for the negative element. The chloride is cast into cylinders about one-third of an inch diameter and 2¼ inches long, with a length of flattened silver wire fused into the centre throughout their length—their weight is about 200 grains. The cell is of glass, one inch in diameter and 5½ inches high, and the wire passes by the side of the cork or stopper, and reaches to the bottom of the cell. The rod of chloride is protected from contact with the zinc element by a sleeve or tube of parchment paper, sewed with thread, which surrounds it, but it is very essential that even this paper should not touch the zinc element, which is simply a small rod of zinc passing through the stopper, and reaching to the bottom of the cell. The stopper is made of paraffine wax. The solution used is composed of about 200 grains of chloride of ammonium to the pint of water. The battery has a resistance of from three to four ohms, but after a time oxychloride of zinc becomes deposited on the zinc element, and increases the resistance greatly—sometimes to as much as 30 or 40 ohms; it is, however, readily removed by placing it in water acidulated with one-fiftieth of its weight of hydrochloric acid, or by adding a minute quantity of acid to each cell. This battery has no local action, and its durability is very great. Mr. De La Rue has many of them in constant use, which have been at work for more than three years, and if not worked they would probably last

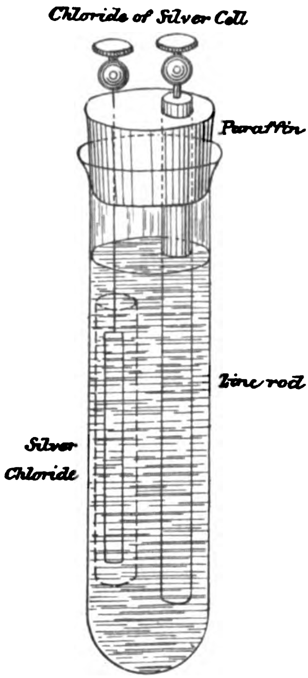


Fig. 1.

an indefinite time. Mr. De La Rue has now eleven thousand of these elements at work at his laboratory in Charlotte Street, and will shortly have fifteen thousand. The effects he obtains with this magnificent battery are of course most striking; he obtains a spark in the air between two terminals at a distance of six-tenths of an inch, and with large charged condensers the explosions he obtains are almost deafening.

By its means he has been able to successfully investigate many questions connected with the length of the spark, the stratification of the discharge in vacuum tubes, and other matters, and he has communicated some of his results to the Royal Society in a paper dated August 25th, 1877.

The first cost of this battery, if made of a size suited for telegraphic use, is of course considerable, but the cost of its use in the long run is not so—the silver is almost all reduced during the lifetime of the battery, and may be completely reduced by putting the waste elements into weak hydrochloric acid with a rod of zinc; the residuum, which is metallic silver, is readily saleable to the metalwrights, or by solution in nitric acid and precipitation by common salt may again be converted into chloride: the waste attendant on this process is very small. Mr. De La Rue has found the loss of silver in his own case to amount to only 1·38 per cent.

The merits of the chloride of silver battery were I think first pointed out by Dr. O'Shaughnessy in a little work entitled "The Electric Telegraph in British India," published in 1853; he there speaks of it as invented by himself and not hitherto described, and he appears to have employed it for telegraphic purposes.

The battery is also used in France for medical purposes. Dr. De La Rue appears to have discovered the merits of the battery quite independently, and it will be in the recollection of most of us that he was kind enough to attend here in April, 1875, and explain its construction and advantages. His remarks will be found in the fourth volume of the Society's Proceedings, at page 202.

Whatever may be its merits when used for telegraphic purposes, I am confident it will be found a valuable battery for testing-purposes and for use on board ship; its great permanency, the constancy of its electromotive force, the absence of all local action, and the facility with which it may be constructed in a portable shape,

are all recommendations, and it has the peculiarity that shaking or striking it does not alter its electromotive force, or cause kicks, as they are termed, an advantage which will be at once appreciated by those who have to test long cables. It does not polarise when joined on short circuit.

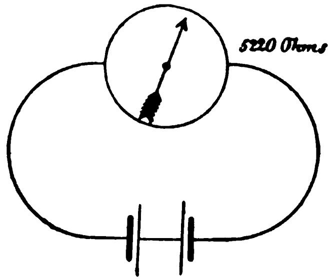


Fig. 2.

The tests which Dr. Muirhead has given in the table were taken by comparing all the cells in succession with a particular cell chosen as a standard, marked C, which is now on the table before us. The results as given in the table represent the deflections produced on a Thomson's galvanometer having a resistance of 5220 ohms by the respective cells when successively joined up in opposition to the standard cell C.

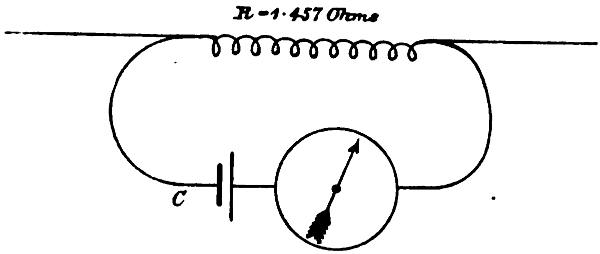


Fig. 3.

Those numbers which have no sign placed to them are negative. The standard cell chosen was rather a strong one, and it will be therefore observed that by far the larger number of cells are negative, or have a smaller electromotive force than the standard. The constant of the galvanometer or the hypothetical deflection which it would give when joined up in short circuit with the cell was equal to 346,730 divisions, and from this value it will be found that the maximum difference between any two cells was in the first tray $\frac{1}{80}$th of the whole electromotive force, or 123 per thousand. In the second tray $\frac{1}{119}$th or 84 per thousand, and in the third tray $\frac{1}{47}$th, or 213 per thousand. This degree of uniformity does not compare at all favourably with that of the Clark's standard element, which in careful hands never shows a variation exceeding one per

thousand. In fact Dr. Muirhead, who has had occasion to prepare many hundreds of these cells at different times, assures me that it is extremely rare to find a difference of potential among any number of them greater than $\frac{1}{2000}$th part of the whole electromotive force and generally not more than $\frac{1}{5000}$th.

At the time I was making investigations on the standard cell above alluded to I experimented on the chloride of silver battery, using the chloride in the state of powder, but though it had obvious merits it did not give such promising results as the sulphate of mercury element, the desideratum at that time being simply to obtain the most uniform standard of electromotive force without reference to any other consideration whatever. It may possibly turn out however that the chloride of silver cell, if not so precisely uniform as the mercury element, may prove better adapted to the practical requirements of telegraphists. It would have one convenient qualification for use as a standard, inasmuch as its electromotive force is practically one volt, or, to speak more correctly, about 1·065 volt.

It is the intention of Dr. Muirhead and myself to make a further investigation of the cell, and to ascertain how far and under what conditions it is possible to obtain a more perfect uniformity of electromotive force, and also to determine its exact value in absolute measure, making at the same time a re-determination of the value of the mercury element. The results will probably be communicated to this Society, and should we be able to supersede the mercury standard none would welcome the result more than myself, for being, as we are, well supplied with practical standards of electrical resistance and electrostatic capacity, we only require a good standard of electromotive force to make our system of measurements complete.

I will take this opportunity of making a few remarks upon the Clark's standard element. In June 1873 I communicated a paper to the Royal Society "On a Standard Voltaic Element;" that paper has never appeared in the Society's Journal,* but a short account of the battery was given in a paper I read to this Society in January 1873, on the Measurement of Currents, which will be found in the

* Now printed, see p. 85.

second volume of the Society's Proceedings, at p. 25. The battery is formed of a paste of mercurous sulphate made by boiling the sulphate in a saturated solution of zinc sulphate, mercury being used as the negative element and pure zinc as the positive. As I have before stated, its constancy or uniformity of electromotive force is all that can be desired, and it has been found to remain in good order for from fifteen months to two years, but after that time the paste dries and the force changes in the form in which it has been hitherto made. It is very liable to injury from carriage owing to the loose mercury in the bottom of the cell. Dr. Muirhead at my suggestion is now preparing these in a greatly improved form. The mercury paste in a liquid state is placed in a bottle by itself and the mercury also. A test tube with the platinum wire soldered in a glass tube, and the zinc rod cemented into a paraffin stopper, are also packed separately. When the battery is required for use, a portion of the paste, previously warmed to ensure saturation with sulphate of zinc, is poured into the tube with a little mercury, the whole is boiled to expel the air, the stopper is inserted while yet warm, the paraffin softening and insuring an air-tight junction, and the whole is ready for use. It is well also to fuse a little paraffin on the top of the paste. When the cell is exhausted another may be prepared in the same way. By this means we obtain complete portability, and the element is freshly

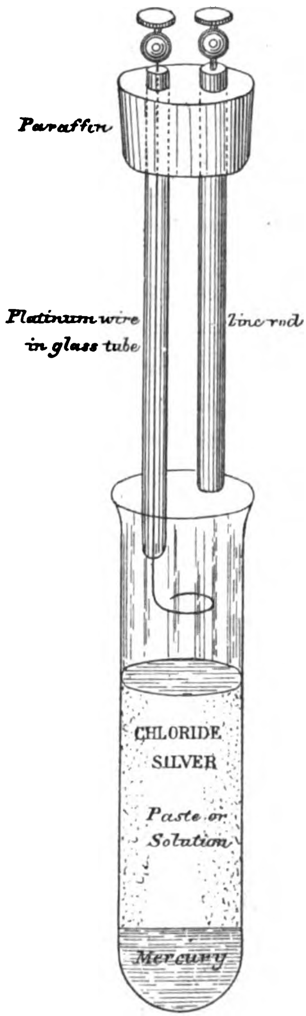


Fig. 4.

set up whenever it is required. Its electromotive force is 1·456 volts. The essential points to remember are that the solution should be boiled for a long time to expel the air. A little free mercury should be added, and if it gets mixed with the paste it does not harm. Thorough saturation of sulphate of zinc is necessary, and if crystals are deposited in cooling they do not matter. There should be no acid reaction, and the protosulphate of mercury should be quite free from persulphate, the presence of which would be indicated by its turning yellow when wetted. The zinc and mercury should be quite pure. The mercury precipitates on the zinc in the form of a fine powder. It is not essential to use the paste, as the saturated liquid poured off from it gives the same electromotive force. With these precautions it is easy for anyone to prepare the elements for himself; but I would remark that although they have been usefully employed for testing purposes they were originally designed only as a standard of electromotive force.

If it turns out that this makes a good practical standard it will have the advantage of being one volt, which is what is required. We are already provided with standards of resistance and electrostatic capacity, but we speak of electromotive force in terms of the Daniell cell because we have no standard to which we can readily refer.

Mr. Ladd: I think, in justice to Mr. De La Rue, I should notice an observation which has been made with reference to this battery being made in Paris. Mr. De La Rue read a paper upon it in Paris, and after that it was brought out and manufactured there by M. Gaiffe; I think it is due to Mr. De La Rue to say that.

Mr. H. Edmunds, Junior, then exhibited the action of Byrne's "Pneumatic Battery."

Mr. W. H. Preece: Mr. H. Edmunds, the agent for this battery, has asked me to make a few remarks upon it, inasmuch as I have been making some experiments with it. The battery itself is a simple cell of platinum and zinc, excited by a mixture of 12 ounces of bichromate of potash with one pint of sulphuric acid and five pints

of water. Batteries of this form are well known, but the peculiarity of this battery is, in the first place, the negative plate is compound; the platinum plate is furnished with a backing of copper, from which it is separated by sheet lead, to improve the conductivity of the plate. In the second place air is pumped into it, in fact it is a pneumatic battery, and during the time of action a small air-pump forces the air through the liquid, stirring it up actively. The original form of battery shown here on the last occasion has been converted by Mr. Ladd into a much larger battery. [Explaining on battery.] There are 10 cells, and these are connected with two brass standards, which are connected together by a piece of No. 14 platinum wire. You will see [illustrating] when the air is pumped the wire gradually gets heated to an intense degree, and on ceasing to pump it gradually cools down; and on pumping again the heat gradually accumulates.

There are here 30 inches of No. 14-gauge platinum wire, and I do not think any member here present has ever seen so thick a piece of platinum wire made hot by any battery. When using 70 or 80 Groves' cells 18 or 24 gauge wire is generally used. You see the great heat that this battery excites. There is nothing that can more indicate the enormous strength of the current produced by it; but not only has this platinum wire been made red hot, but these No. 10-gauge copper wires used as connecting pieces are so hot that I cannot hold them. No doubt if the connecting-wire were thicker the heat would have been greater in the platinum wire.

This battery was introduced for medical purposes—for cauterising; an interesting case of which has been recently performed in London by means of it.

Now, the first question that arises in the mind of the electrician on examining the working of this battery is—Is this strange action due to chemical action or is it due to mere mechanical action? Why does the pumping of the air into the liquid produce these marvellous effects?

To see whether they were due to chemical action, Mr. Ladd pumped through one of these batteries, first oxygen, then air, then hydrogen. Oxygen being a more exciting agent than air, if it were due to chemical action we should imagine the effect would be

increased—but there was no difference; whether oxygen or air or hydrogen was pumped in the effect was the same. Hence we conclude that the effect is not chemical but mechanical.

Now, if it is a mechanical action, what does it do? Does it increase the electromotive force of the battery, or does it reduce its resistance? It must do one or the other. The increase of strength of current is due either to increase of electromotive force or to diminution of resistance. To prove whether it was due to increase of the electromotive force I first measured the electromotive force of a cell when quiescent, then with air pumped in, and, lastly, after it had been in action for some time. I found under these three conditions the electromotive force was exactly the same. The electromotive force of the cell was equal to 1·7 of a Daniell cell. We may say the electromotive force of this battery is the same whether quiescent or with air pumped in. This rather tends to upset the theory proposed by Mr. Byrne himself, that the action of the battery is due to the depolarization of the negative plate. The polarization of the plate would have the effect of diminishing the electromotive force, but inasmuch as it does not alter the electromotive force in any degree we may say it is not due to depolarization. To what then can it be due? It may be due to the withdrawal of the reduced salt from the negative plate and its replacement by fresh solution, but, if it were due to this alone, it would have an instantaneous action; it has not; therefore it cannot be said to be due to that cause.

One striking effect of the battery is, that, with the heat which you now see externally, we find a great production of heat internally, and as we work up the battery we find the plates of the cell so hot that they cannot be handled. I am therefore inclined to think that there is some action in the interior of the battery that tends to produce heat, and that this heat reduces the internal resistance of the battery. The resistance is so small that we have no means of measuring it. While the electromotive force of this cell is 1·7 volt, the internal resistance is infinitely small, and hence we have these wonderful effects. It is a very interesting and beautiful battery, and no doubt for medical purposes will surpass any battery now in use.

Professor ADAMS: With regard to this battery, I think the question is whether the greater current produced is not due to the circumstances of pumping in the air, so that fresh acid is constantly coming in contact with the zinc plate, and therefore the action is kept up; otherwise from the liquid being in contact with the plate there would be at once a greater resistance. There is no doubt, under these circumstances, we have a much greater chemical action accompanying the current. I should like to know how far the experiments can be taken to bear on the point whether the current is due to chemical action. We shall have greater action accompanying this current in consequence of so much fresh acid being brought into contact with the surface of the zinc, though the electromotive force is exactly the same. Of course the resistance of the battery is diminished in consequence of the introduction of fresh acid to the zinc plate.

Mr. PREECE: The argument I use, as to the assumption that it was not due to fresh acid, is based on the fact, that, if it were due to fresh acid on the plate, it would have an instantaneous effect; but it is a curious fact that it is a cumulative action, and you have to pump a long time before you work up to great heat. If it were due to fresh acid pumped upon the surface of the negative plate, I think we should have a more rapid effect.

Mr. R. GRAY: Can you say whether, for a given quantity of oxygen pumped in, the heat was greater than from a given volume of air?

Mr. PREECE: The experiment was made to show whether increased effect arose from the introduction of oxygen, and it was found that whether oxygen, or air, or hydrogen, was used, the effect was just the same. There was no measurement made of the heat either outside or inside.

Mr. LADD: From what experience I have had I think there is a good deal in what Professor Adams says. I think there is a good deal in the fresh acid brought into play against the plate. It would take some time to make a large platinum wire hot. It takes time if put into flame. So here you must keep on pumping, to get up the effect; but I think there is a good deal also in the plate. The copper plate is a conducting plate. It is covered with

lead, for the purpose of protecting the copper and the solder, because it has to be soldered to the copper, and it protects the copper effectually from the effects of the acid. I find that at first I tried to do away with the lead and soldered the platinum on. I tried to do that upon the copper, but I found the acid acted upon the solder, and left the copper, though it was well tinned. Still it did act upon the tin and laid it bare, and then it gave a local action. The copper was eaten away in the thinnest part. I think the conduction of the copper has a great deal to do with it. If that was not the case I think you would get the same effect with carbon plates, but you do not. Carbon is a bad conductor of electricity, and the consequence is you do not get the whole power out of the battery which you might if carbon was a better conductor. Here the copper is brought up in close connection with the connection at the top, and that conducts the electricity freely away. The wires I think are No. 9 or 10 Birmingham gauge, and they got quite hot. The light shown to-night is a very bad one—for this reason, it has not been tried with that battery till to-day; but underneath this stand there are wires not more than No. 14-gauge. If these No. 9 wires get so very hot what must the No. 14 do? And that retards the current through this circuit, which exceeds the power got from what I expected with these 10 cells.

Professor ADAMS: Is the plate exposed to the liquid or backed by varnish?

Mr. LADD: The back of the lead-plate is varnished to protect it. The platinum is very thin, and is soldered on to the lead, with the surface towards the zinc. The copper is entirely covered with lead both sides, and then the thin platinum foil is soldered on the lead, and there are two platinum plates to one of zinc.

Mr. EDMUNDS: Dr. Byrne, who has experimented with the battery, attributes more advantage than we do to the fact of reducing the resistance of the negative plate. He does not think it is due so much to the mechanical action of the air as to that reduction of resistance of the plate.

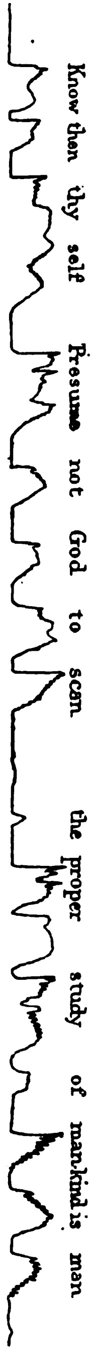
The CHAIRMAN: I agree with Mr. Preece that great merit is due to the small internal resistance of this battery, which is said to be so small that it cannot be measured. I would submit that

experiments might be tried of an analogous kind, by moving the liquid with a brush, or other mechanical means, to see if it has the same effect as the gases. I am sure you will all agree that we should return our thanks to Mr. Edmunds for his kindness in showing us this battery.

A vote of thanks was unanimously accorded to Mr. Edmunds.

Mr. W. H. BARLOW, F.R.S., V.P. Inst. C.E., then gave a description of the Logograph. He said—The small instrument I have to show you is not altogether a new one. It is scientifically getting rather aged—about four years old. This instrument, I may confess at once, owes its origin to an attempt to make something which would do short handwriting instead of employing the services of the gentleman who sits there. We have failed in doing that, but it has certainly served some purpose. It has served the purpose of illustrating certain features connected with the articulations of the human voice. Everybody knows that, in speaking, the air contained in the lungs is sent forth from the mouth in spurts of different magnitude and with different degrees of velocity and intensity. I may say that forty years ago I first thought of this when I saw an old Turk smoking his pipe. The Turks, as you know, inhale the smoke into the lungs, and, as they speak, you can see all the action of the air coming from the mouth. I was also informed of the extraordinary sensitiveness of some of the instruments used for registering indicator diagrams of the steam engine at very high velocities, and it occurred to me to put these two things together and form a delicate apparatus, and so obtain indicator diagrams showing the relative powers of the different *syllables* uttered by the human voice.

The instrument I formed was that now before you. It consists of a small speaking tube, the end of which is enlarged to a disc of about 2½ inches diameter. That end is covered with a piece of thin india-rubber, taken from a toy balloon. That membrane being stretched with a certain degree of tension, a light aluminium arm is made to press against it, the end of the aluminium arm holding a small sable brush containing colour; the speaking part of the instrument is, to a certain extent, similar to the telephone; the



other part of the instrument causes a ribbon of paper to pass under that brush, and then speaking into the instrument you get a diagram of the utterances that are made.

[Mr. Barlow illustrated this—the mechanical action of the instrument being similar to that of the tape-printing telegraph—and passed the ribbon on for the inspection of the meeting.]

You see there (Mr. Barlow continued) the action produced by the sounds I have just given. I may mention some facts that I have obtained as to the absolute quantity of air consumed by this process. The horizontal movement is about half an inch for some of the letters, and the whole diagram is a very large and conspicuous one. I found on an average of experiments the quantity of air consumed for each syllable, or rather the average quantity of air for each syllable, is 1½ cubic inch. It varies very much in different syllables. The quantity of air consumed was ascertained by taking a balloon of the cubical area of 513 cubic inches, and I found it took an average of 359 syllables to fill it. That brings it to an average of 1½ cubic inch for each syllable. I also found the capacity of the ordinary breath is about 40 cubic inches, and that by taking an extreme breath you may bring up the quantity to 80 or 90 inches. I also found that you can speak against a pressure equal to that of two inches of water, but against a pressure of four inches it seems impossible to speak at all.

Having made these experiments, which resulted in showing the peculiarities of articulation, and how the different letters were formed, and what happened to them, it occurred that the marker I was using became fixed to the disc. That arose from having used gold-beaters' skin, which caused the attachment of one to the other. I found the consequence was to produce a vibration from the vowel sounds. [Mr. Barlow directed attention to a diagram on the wall illustrating

the vibratory action of vowel sounds as distinguished from the pneumatic action of consonants, and went on to remark.] These larger lines represent the consonant actions. You observe that following them is a fine vibration, and that vibration is the vowel sound, or rather I believe there is a certain organ which vibrates whenever the vowel sound is uttered. I think it is a kind of drone which is present whenever the vowel sound is uttered. A whisper is sufficient to make known the vowel sound intended, whereas whispering will make no vibrating action. The form of the diagrams produced leads to a good many curious questions, as to the action of vowels and consonants and compound letters, things which will combine and those which will not, and I have no doubt those who know something about the anatomy of the throat and the laws of sound will be able to get more out of this instrument than I have succeeded in getting.

The CHAIRMAN asked whether, in repeating the same words over and over again, the signs recorded are constant.

Mr. BARLOW: They are constant, subject to very minute variations that arise in this kind of thing. It is almost impossible to speak the same words exactly in the same manner each time. This instrument is purely phonetic, and however minutely your articulation differs at one time from another that difference is registered.

Illustrations were given of the diagram produced by the trilling the letter "r," also by repeating the same words two or three times.

The CHAIRMAN said he was struck with the great extent of the movement of this instrument. The movement was quite half an inch in some of the longer terms. It seemed to be necessary to place the mouth close to the tube.

Mr. BARLOW explained that it was necessary to do so in order that the full effect of the air might be brought into operation.

The CHAIRMAN: In other kinds of instruments of this description you do not place your mouth so close to the tube, but depend more upon the vibratory action of the air.

The thanks of the meeting were then unanimously accorded to

Mr. Barlow for his kindness in exhibiting his most interesting invention.

The CHAIRMAN then called upon Mr. W. H. Preece to give his promised description of

"THE PHONOGRAPH."

Mr. PREECE said: The instrument which has just been described to you by Mr. Barlow is a very fitting prelude to this wonderful instrument that has just come to us from across the Atlantic. The phonograph of Mr. Edison is an instrument which is constructed for the production or rather reproduction of sound by mechanical means. It differs from the telephone in this—that the telephone is an instrument that produces sounds at distant places by the aid of electricity; but in the first principles of the two instruments we have the same identical effect, viz.: that by producing vibrations by the voice upon a disc placed in a certain position we are able to produce effects which reproduce those sounds.

Now, in order to make this plain to you, I have again called in the kind aid of Mr. Edmunds to produce before you the experiments which I had the pleasure of bringing before the Royal Institution. I have here what is called a Reiss' transmitter. At the end of this nickel-plated brass tube is a diaphragm of india-rubber, and whenever I speak into that tube the india-rubber disc vibrates. You can hear it vibrate by the two plates at the contact points coming into contact. Whenever those two plates come into contact with each other they complete the circuit of a battery which passes through the primary coil of an induction coil, and the secondary circuit of that coil passes through a vacuum tube. This vacuum tube rotates by means of a common electro-magnetic mill worked by three Groves' cells. If the lights are put down we shall see the sparks that pass through the vacuum tube, and as different notes are sounded into this transmitter these different notes will be reproduced in different figures.

You will observe that every note has its distinct figure.

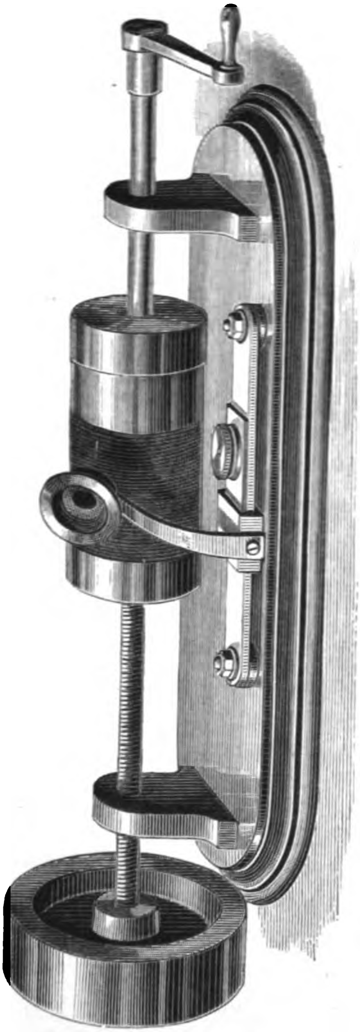


FIG. 1.

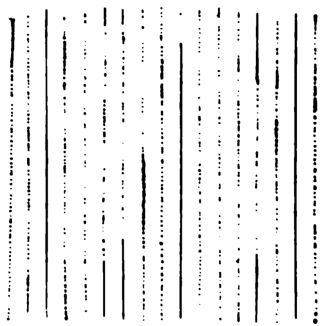


FIG. 2.

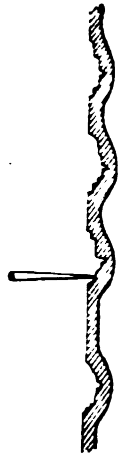


FIG. 3.

Now the evidence we have there is, that, whenever this disc is placed in front of the mouth, the sonorous vibrations issuing from the mouth, or from a musical instrument, make in that instrument a different figure.

Now in the phonograph we have a precisely similar disc—a disc identical in form and in character with the disc used in the now well-known telephone; and at the back of the disc there is a steel point which, by means of a spring, gently presses against a piece of tinfoil. Here is one of the pieces of tinfoil that is used. This tinfoil is placed around the circumference of a long cylinder, which cylinder has a thread upon it—a spiral thread—and the length in this case of this thread is about 42 feet. Now this cylinder is rotated in this particular instrument by a handle, and in order to give the cylinder a uniform rotation at the extreme end of the axle carrying the cylinder is a heavy fly-wheel. I may tell you this instrument is only the second which Mr. Edison, that wonderful genius on the other side of the Atlantic, has made. (See fig. 1.) It was brought over from America by my friend Mr. Puskas, who has brought it here, and who, as he understands the working of it better than I do, will show it you in actual operation. Now here we have all the simple conditions of a phonograph. We have a disc vibrating in response to the tones of the voice. We have a sheet of tinfoil, a yielding substance, which receives the impressions, and being highly non-elastic it retains those impressions, so that when you take up the sheet of tinfoil you will find, as far as the eye is concerned, little indentations, little rapid dots in telegraphic language, but really, as far as the eye can see, nothing but a series of these dots. (Fig. 2.) However, there is no doubt that these dots on the tinfoil follow exactly all the conditions of the sonorous vibrations expressed by the voice. Now, while I am speaking to you, vibrations enter the ear in the form of words, which strike the tympanum of the ear—an instrument identical with the mouthpiece of the phonograph. The tympanum of the ear vibrates responsively to the tones produced, which in the phonograph is imitated by the steel point at the back of the disc upon this tinfoil, so that if we take a succession of tinfoils we shall have a succession of undulations similar to

those of Mr. Barlow, only they are more minute. The marks on the paper of Mr. Barlow are very large; but these illustrations upon the tinfoil only vary a small fraction of an inch. (For magnified section, see fig. 3.)*

This impresses upon the tinfoil the marks as you see. The number of these waves or vibrations determining the note given out by the voice, the depth of the vibrations giving the loudness of the tones, and their form giving you the quality of the notes produced —whether it be the deep bass of a man's voice or the soprano of a woman's voice—whether it is a trumpet or a whistle,—every single sound imparted to that disc imparts a curve on the tinfoil, and that curve is the exact representation of the sounds. The result is, after having spoken into the cylinder you have the foil on the cylinder indented with these waves, and all you have to do to reproduce those effects is to cause these indentations on the tinfoil to go over the same course again; but in the second result, instead of being produced by the point of the disc these curves impart motion to the point, and the point imparts motion to the disc that previously vibrated; and if the motion be the same and at the same rate, and if the point goes over the same ground, tumbling into these indentations and gently riding over the hedges and ditches, the result is the disc is caused to vibrate exactly in the same way as it was caused to vibrate by the voice, and the consequence of this repetition of the vibrations is the reproduction of the same sounds.

Mr. Puskas will perhaps go to that instrument and say something to it.

[Mr. Puskas complied, and spoke as follows:—"The Phonograph presents its compliments to the company," which was repeated by the instrument with great distinctness.]

Now (continued Mr. Preece) this instrument which you have just heard is only the second of the kind ever made. We succeeded in England in getting some very careful written descriptions of it, and my friend Mr. Pidgeon, of Putney, a very able amateur mechanician, has, with the aid of his son, succeeded in producing a fac-simile of Mr. Edison's. The only difference between its construction and

* The Society is indebted to the courtesy of the Proprietors of "Engineering" for the engravings of the Phonograph.

To face page 71.

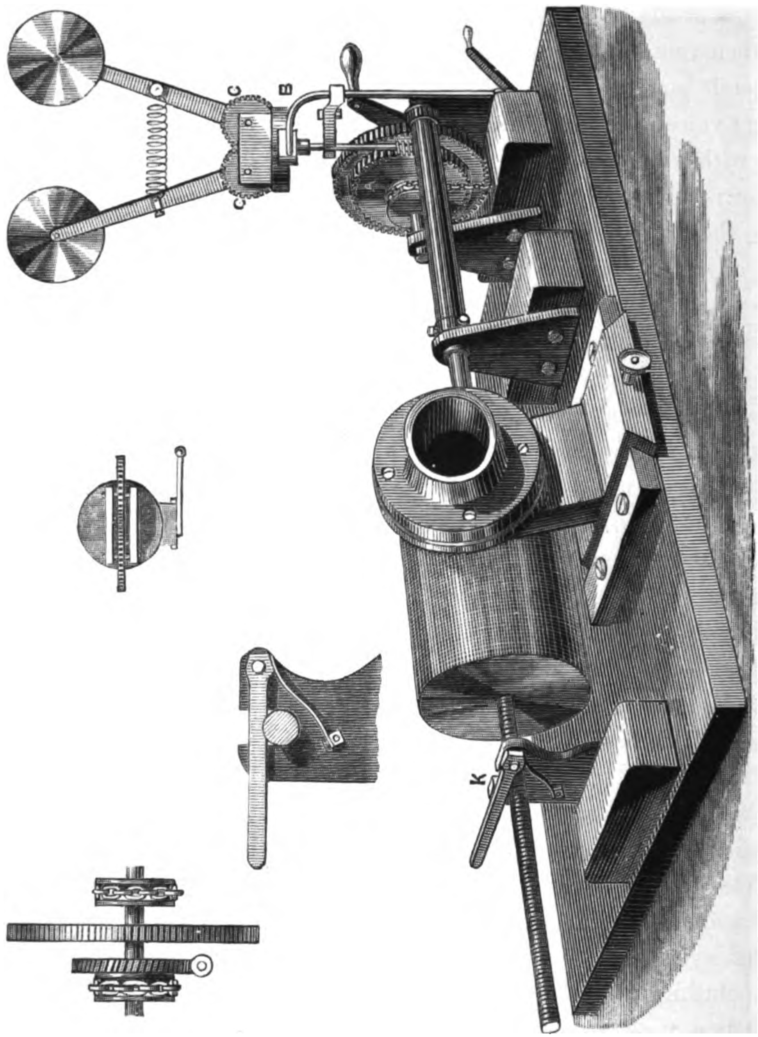


FIG. 4.

the one you have just seen is that the diaphragm into which the words are uttered is different from the one which reproduces the sounds. There are two diaphragms, and I am not sure that in abandoning his first plan Mr. Edison has made an improvement.

The great advantage, in my opinion, in having two diaphragms is that when you have a disc for receiving sonorous vibrations from the mouth you can have it set so as to be equally resilient to different voices. A strong voice or a weak voice can equally speak into it without affecting too actively the tinfoil; but that which is good for receiving sonorous vibrations is not equally good for imparting them. With a different diaphragm for receiving, it is a matter of indifference what loudness of sound is imparted. It will always reproduce the same sounds with the same amplitude. Hence you are able to make your sounding disc of lighter and more spring-like materials than your receiving disc, and you have no fear of damaging the tinfoil with loud words.

Now, Mr. Pidgeon has kindly brought his instrument here to-night. This is not to be regarded as a competitive exhibition. Here is the second instrument made by the inventor himself, and here also is the first that has been made by an amateur from a description of the inventor's own instrument. Mr. Pidgeon and his son will now exhibit that instrument to you. [Illustrated—several sentences were given and songs sung]. I am sure you will congratulate Mr. Pidgeon upon such a very excellent example of amateur mechanical skill. There is one defect in it which led Mr. Pidgeon rather to deprecate the singing of songs—that is, they come out rather out of tune. The reason is this—with the mechanical contrivance for rotation by hand it is almost impossible to produce that uniform motion which is necessary to reproduce notes in their exact pitch. Mr. Edison has endeavoured to get over that by the addition of a heavy fly-wheel; but in this instrument [pointing to another] that difficulty has been got over by the introduction of clockwork and a governor. (Fig. 4.)

Some three weeks or a month ago I had the honour of lecturing before the Royal Institution, and, the phonograph being then a greater novelty than it is now, I had been promised by Mr. Edison an instrument for exhibition, but, knowing that inventors cannot

always fulfil their promises as to time, I thought the better plan would be to secure the services of one of our ablest mechanicians, Mr. Stroh, to undertake the manufacture of an instrument from Mr. Edison's description. He did so, and this instrument was exhibited at the Royal Institution. It reproduced sounds in a very admirable manner, but we found the same evil—the impossibility of reproducing the exact pitch of the notes, owing to the irregular motion of the apparatus when moved by hand. So Mr. Stroh has constructed for us the apparatus you see here. It is in principle the same, but he has applied to it a weight and an ingenious governor, which secures the uniform motion that we want. It is still, however, of course, Mr. Edison's instrument. We have the disc to speak to, the cylinder with its spiral thread, the tinfoil, and everything you have seen before, but we have, in addition, that uniform motion which I hope will enable us to reproduce musical notes in a way superior to what we have yet heard.

[Mr. Spagnoletti sang into the instrument a stanza of a popular song, which was reproduced by the phonograph, and on being encored was again distinctly reproduced by the instrument. Mr. Preece also sang with similar results.] Now, Gentlemen, while Mr. Stroh is putting in another sheet of tinfoil I may mention that this inventive genius, Mr. Edison, has devoted a great deal of time to the perfecting of this instrument. His later form, instead of having this foil spread on the cylinder has it spread upon a flat disc, and the disc rotates so as to bring the foil continuously underneath the point in the form of a spiral, so that instead of having these marks upon the circumference of the cylinder they would be recorded upon the spiral curve, and he has also applied mechanical power to secure uniformity of motion. I am told that in his new instrument he has produced sound sufficiently loud to be heard at a distance of 425 feet, and he has so improved the articulation that the dictated matter was repeated at Philadelphia twelve times without mistake. The articulation is perfect, and even whispering is repeated. The sounds can be reproduced not only once, but a great many times. Take one instance. I called on Mr. Puskas immediately on his arrival at the Langham Hotel. He turned the phonograph. It said, "How do you do? what

do you think of my phonograph?" These words had been imparted to it by Mr. Edison in New York before its departure for England; they had been reproduced several times *en route* for the gratification of the passengers on board the steamer, and were again made distinctly audible to me at the Langham Hotel a fortnight after they had been originally uttered. (Applause).

The instrument itself is so simple that the dullest comprehension cannot fail to understand it.

[Mr. Spagnoletti here sang into the instrument "God Save the Queen," which was distinctly reproduced.]

Mr. PREECE remarked that there was no reason why Mr. Spagnoletti's melody should not be kept for one hundred years, say, till 1978, when the sounds would be reproduced as "God Save the Queen, sung by Mr. Spagnoletti before the Society of Telegraph Engineers in England in 1878." (Laughter.) There is just one more effect I should like to produce, though it is almost a desecration to superpose a song upon what we have just heard; nevertheless it will be worth your while to hear the effect of sounds superposed on other sounds. [This was illustrated, the two different sounds being quite distinct though simultaneous.]—Before I sit down I beg to convey my best thanks to Mr. Puskas for the loan and exhibition of Mr. Edison's instrument; to Mr. Pidgeon for bringing his clever workmanship here; and to Mr. Stroh for the exhibition of the beautiful instrument he has been kind enough to bring here and manipulate to-night.

The CHAIRMAN: It is now past our usual time for adjourning, but I am sure you would not wish to separate without an expression of your thanks to the gentlemen who have occupied us with an entertainment so agreeable as we have experienced to-night, or without conveying to Mr. Edison at New York an expression of the great pleasure which the exhibition of his wonderful instrument has afforded us. I regard this invention as an epoch in the history of science, and we ought not to let this occasion pass without recording a vote of thanks in return for the great pleasure we have had this evening in hearing Mr. Edison's instrument, and I hope that vote will be communicated to him by telegraph.

Carried by acclamation.

Mr. WEAVER said he would undertake to telegraph the communication to Mr. Edison, an offer which was gladly accepted by the meeting. A cordial vote of thanks was also passed to Mr. Puskas, Mr. Pidgeon, and Mr. Stroh, for the exhibition of their respective apparatus, and to Mr. Preece for his lucid description of them.

It was announced that Mr. Conrad W. Cooke had been transferred from the class of Associates to that of Members.

The Meeting then adjourned.

ORIGINAL COMMUNICATIONS.

GASTON PLANTÉ'S NEW ELECTRICAL MACHINE.

The beautiful experiments of Mr. Warren De La Rue are well known in England; I was fortunate enough last year to see in his laboratory sparks produced by 1,080 and by 5,640 elements of his chloride of silver battery.

I saw also long Geissler-tubes lighted, lighted by the constant flow of the current produced by that enormous battery.

We have now in Paris, in the laboratory of my friend M. Planté, a source of electricity which stands comparison with Mr. Warren De La Rue's pile. M. Planté has now in action 800 secondary cells. You remember that the electromotive of these cells is equal to 1·5 Grove, or to 2·93 volts; in round numbers we may say 3 volts. Consequently the 800 cells of Planté have an electromotive force of 2,344 volts, or very nearly 2,344 chloride of silver elements.

Mr. De La Rue having been able to make many of his experiments with 1,080 of his cells, you see that Planté can do a good deal with his 2,344 volts.

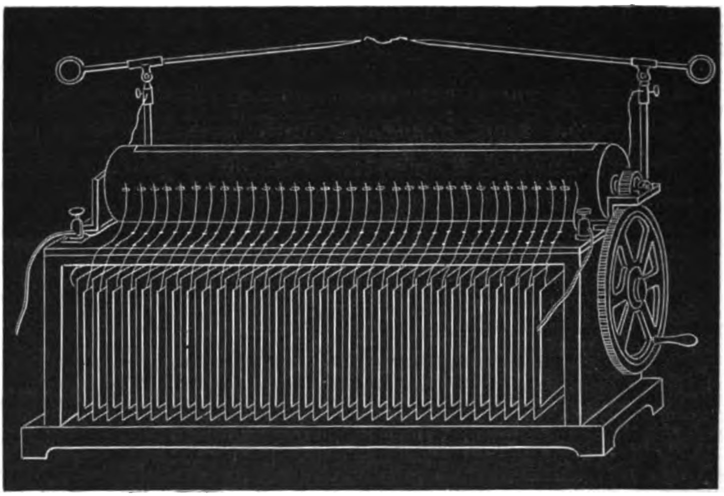
You know that the electromotive force of the secondary battery is variable from a maximum which I have mentioned, and decreasing constantly when you discharge it. But the quantity of electricity forced into these cells during the period of charge is really enormous, so that experiments can be carried on for a long time; M. Planté can, for instance, keep a Geissler-tube illuminated for three and a half hours.

When I had the pleasure of seeing the experiments of Mr. De La Rue in his laboratory, I took a note that the stratifications were almost steady. In the experiments shown to me by M. Planté I have again been surprised with the steadiness and fixity of the statifications; I can say that I have seen them perfectly quiet. Another fact which Planté has made me observe is this: the

Geissler-tube soon gets hot after it has been traversed by the current of the battery; and the narrow parts are very much hotter than the parts of a greater diameter. That is certainly not surprising, but the difference is sensible to the fingers, and the experiment is extremely illustrative.

Now Planté has proposed to charge a number of condensers with his battery (2,344 volts), and, when charged, to connect them in intensity, so as to multiply the tension by the number. In other words, he charges a number of condensers connected in quantity, and he discharges them in intensity.

To perform that experiment rapidly he has contrived an instrument, which he calls *machine rhéostatique*, and which you see in the diagram.



A large cylindrical commutator is so arranged that in one position all the left-hand plates of the condensers are connected together by springs touching a bar of copper, and so are the right-hand plates. In that situation the connection is established with the 800 secondary cells, and the condensers are charged. When the commutator turns 90 degrees, the springs are brought to touch a number of separate knobs connected in cascade, as is the

case in the commutator of his secondary battery, and, consequently, the condensers are joined in intensity like the elements of a pile. In that new position the discharge takes place in the shape of a spark, as shown by the figure.

You easily understand that in a revolution of the commutator there are two periods of charge and two of discharge, or, in other words, two sparks per turn.

Planté has obtained with 30 condensers sparks of 4 centimetres. That is very satisfactory, I believe, for a first instrument, and certainly the early induction coils were unable to do that.

You observe that the sparks are, like those obtained from an electrical frictional machine, all in one direction, and not composed of reversed currents as is the case with the induction coil.

The new machine has again an advantage over the coils; the current of the battery is never short-circuited, and all the electricity supplied by it is sure to go in the sparks: while with the Ruhmkorff coil you close the circuit of the battery on the primary wire, and you lose a portion of the current, which it is not easy to determine, but which is considerable.

I have no doubt the *machine rhéostatique* will be improved, but, as it is, I hope the Society of Telegraph Engineers will find it worthy of their consideration.

ALF. NIAUDET.

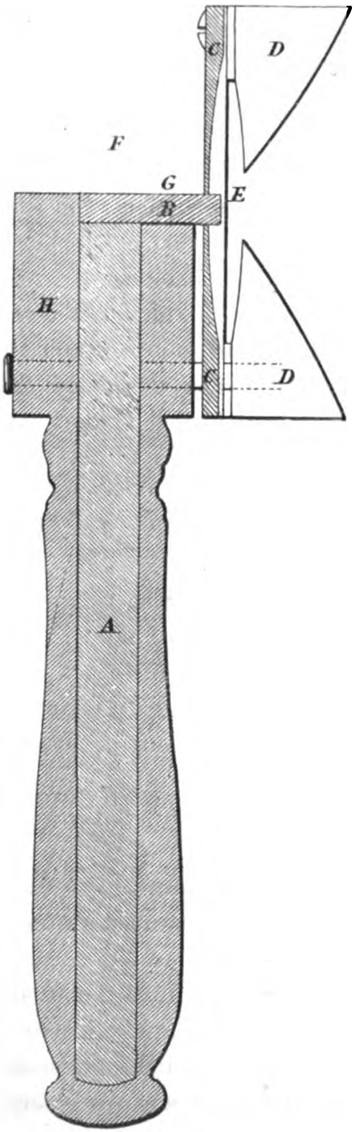
THE WIRE-FINDER.

In a previous communication to the Society I described a simple form of wire-finder.

Such an instrument is a great desideratum, and will be useful in *every* telegraph office. Many defects in working are instantly discovered by its agency, and are therefore quicker and more easily remedied. No connections require to be made, and a wire wherever it can be got at may be "sounded" with the greatest facility.

The applications are so numerous, and so readily suggest themselves, that it is unnecessary to enumerate them. Suffice it to

say that all working currents can be distinguished by it, and a little experience enables one to judge of the strength and kind of signals passing through the several wires under examin ation.



The accompanying figure is a sectional illustration of a simple and handy instrument which I have devised.

A is a round bar magnet passing down the handle of the instrument; on one pole at B is screwed a piece of iron, the end of which passes through a brass plate, C, which is screwed on the ear-piece, D, at the same time binding a thin iron plate, E. The portion F of the under circular piece is cut away, and the wire to be tested is placed against the magnet at G.

The ear-piece with the brass plate and diaphragm is made separately from the other parts, and is screwed permanently on H, in which the handle is fitted. The part F (which is not shown in the figure) is connected to H either by a hinge on the under part or by sliding grooves on the face of H, in such a manner that it may be closed against the wire to hold it in place, and the upper corner of F is cut away at G so as to inclose the wire.

I have found that Prof. Bell's hand telephone will serve the same purpose as the instrument above described if the wire be placed under the mouthpiece (outside the instrument). And another appli-

cation of which this instrument is capable is often more convenient than the one I describe, because with it we can test a wire which from its position may be difficult to get at, as for example if the wire be at the bottom of a trench. Though I should remark that a few yards of rubber tubing applied to the ear-piece enables one still to hear distinctly the vibrations of the diaphragm.

For use with the telephone instrument I recommend a wooden bobbin, an inch in diameter, and one inch and a quarter long, wound full with No. 35 silk-covered copper wire, a soft iron core, a brass tube over the bobbin, and a terminal on each end of the bobbin.

This coil is to be connected by two wires to the instrument. When a wire is to be tested the bobbin is held against it with the coil wires parallel to it. In some cases signals can be heard passing through the wire when this coil is held a foot or more distant from it. Sometimes we can combine this induction coil with the other method, and thereby increase the effects, taking care that the coil is turned in the right direction, otherwise it opposes the direct action upon the magnet.

One of the most important uses to which this apparatus may be applied is in repairs to submarine cables.

With the induction coil described, I can hear distinctly the ordinary working signals passing through a large shore end, twenty tons to the mile. I have heard them on the sea-beach two miles away from the office where they originated.

It remains to discover by future experiment to what distance signals can be heard.

In the case of a broken cable, it frequently happens that a cable is picked up in shallow water in a boat, and it may be desirable to know whether to underrun towards the shore or out to sea for the fault. This apparatus will enable us to ascertain at once on which side the break is, without cutting the cable.

Of course the resistance of the broken end will be taken into account, and experiments made on shore to ascertain the strength of battery necessary to employ on the broken cable, and steps would be taken to insure that intermittent currents are passing unceasingly to line. The quicker these currents succeed each other

the more easily they are heard, and a simple form of whip is best adapted for sending them.

As there would be an advantage in ascertaining the presence of a permanent current instead of intermittent ones only, I accomplished this by constructing a small coil similar to a Siemens armature, and mounting it on pivots, so that it could be revolved rapidly. When this revolving coil was presented to a wire on which was a permanent current., its presence was at once detected on the telephone.

The objection to this method is that strong earth currents would be liable to be mistaken for the permanent current looked for. An objection which could not arise in the other case.

Under favourable conditions it is probable signals might be heard through twenty miles of cable.

JOHN GOTT.

St. Pierre, Miquelon, January 26th, 1878.

AUTOMATIC CURB SIGNALLING APPLIED TO THE ORDINARY HAND KEYS FOR USE ON LONG SUBMARINE CABLES.

ST. PIERRE, MIQUELON,
December 18, 1877.

The time it takes for the hand key to fall and rise is inappreciable, but the time it rests on the bottom stop is comparatively long. The depression which the hand suffers *after* the key first touches the bottom contact has to be *recovered* before the key *begins* to rise. This is the period we have to utilise in sending a main signal and a curb signal.

This is accomplished in the method described in this paper.

The sending-key, fig. 1, as usual, consists of two levers placed one above the other in the paper for convenience of illustration; the top one being the right hand, and the bottom one the left hand key.

They turn on pivots at 1, 1′.

The top lever is divided into two portions insulated from each

other, the contact screws A, B, work in the top bar of the lever: this top bar being permanently connected with the line by a spiral wire under a screw as indicated. The under bar of this key is permanently to earth at 1, and has a platinum contact at 2.

The left key (bottom one in the fig.) carries three contact points C, D, and 2′. This lever is also permanently connected to earth at 1′.

Under each key is placed a spring, 3 and 3′, carrying platinum contact plates at the ends to make contact with B and D. The contacts 2 and 2′ are made with the anvils 4 and 4′.

The curb signals are sent automatically by the apparatus at fig. 2, which is actuated by the hand-key already described. It consists of a lever moved by an electro-magnet and moving between two adjustable screws. This lever turns on a pivot at 5.

Fixed to this lever are two blocks of vulcanite carrying springs E and F, which make contact with the lever itself at G and H, or with the adjustable screws I and K. These screws are supported on arms projecting from a pillar which is itself divided into two portions insulated from each other at L.

The mode of operation will now be readily understood if we trace the connections indicated by the broken or dotted lines.

With the system at rest the condensers on the cable are to earth through the screw A back contact X C and 1′, and this is the condition of things after each signal.

Now, depress the right hand key, the contact A X is broken and B joins the spring 3, which is in connection with F K and the zinc pole of the battery. Copper being to earth through 5 G E X′ and 1′, thus a negative current flows to line, giving a right-hand deflection. Continuing to depress the key 2 connects with 4, closing the local battery circuit through 6 and 7 by putting the local battery to earth at 1. This moves the lever in fig. 2, and the battery is reversed, for now we have cable B 3 F H 5 to copper, and zinc to earth through M R N I E X′ 1′, and a positive current flows to line immediately after the negative. But this reversal of the battery to produce a curb signal does not take place instantly that 2 and 4 meet, otherwise the first negative current would produce no effect at the distant end—the main signal would not be

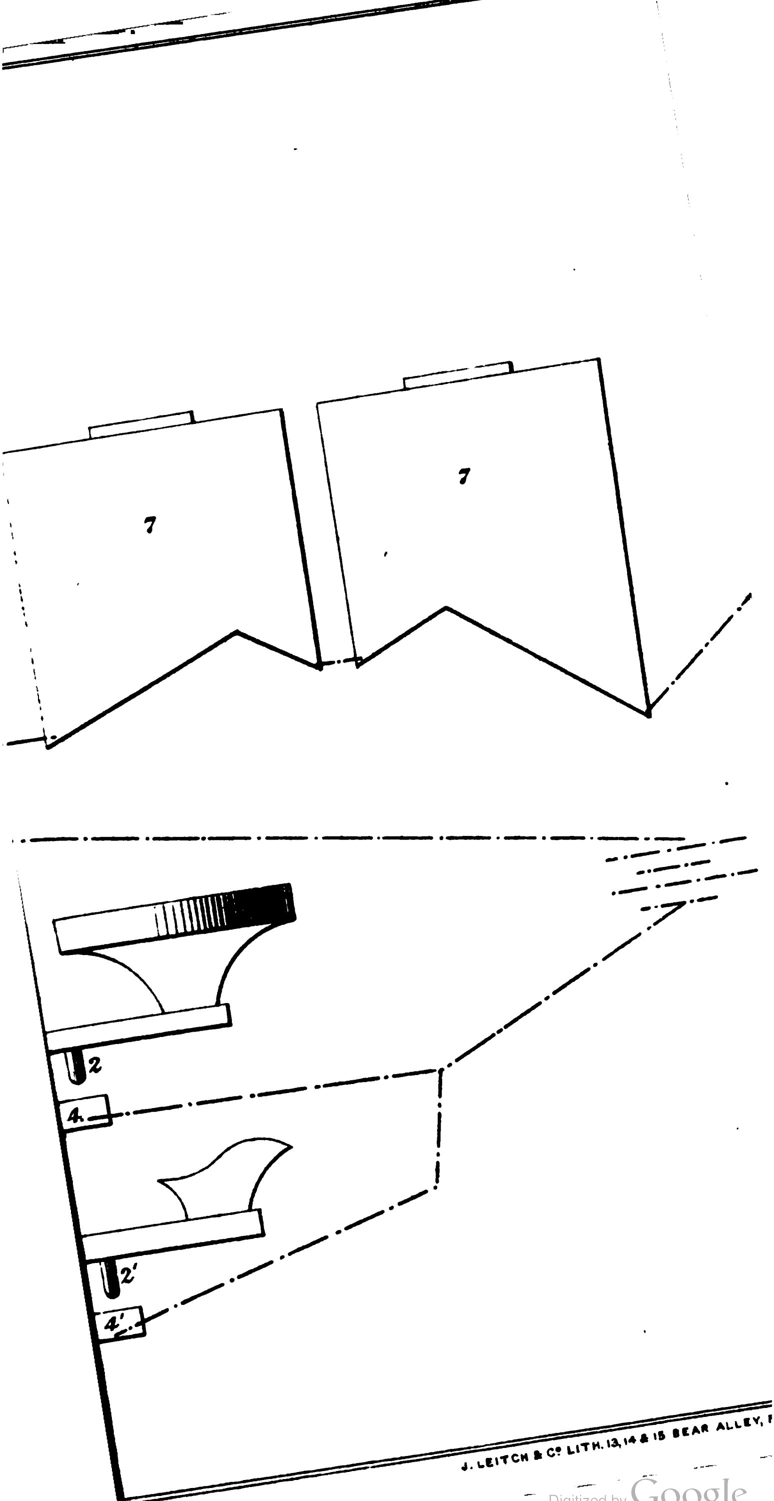
made. The necessary delay is established by placing in circuit the electro-magnet 7 provided with a sliding armature, or otherwise adjustable armature or shunt, to produce an electro-magnetic inertia in the system of a definite value, so that a certain interval of time elapses after the key is depressed before the curb signal is sent. And this curb signal continues to flow to line until the key, being released, assumes its position of rest and puts the line to earth.

A similar change takes place if we depress the left key. At first D connects 3′ F K zinc to E at 1′, and copper to line through 5 G E X A, giving a left hand deflection at the distant end. Then 2′ 4′ make contact, and after a definite interval the lever fig. 2 is depressed, and now zinc to line through M R N I E X A, and copper to earth through 5 H F 3′ D 1′. And the curb signal continues to flow to line until the key, being released, assumes its position of rest and puts the line to earth.

The inertia established in the local circuit must manifestly bear some relation to the speed at which it is intended to work. This adjustment is guided by the ear—it is made for a certain speed of working, which must be fairly maintained; if the speed falls very much then curbing is unnecessary; but the operator is not confined rigidly to a certain speed, a certain latitude such as would be likely to arise is permitted without changing anything. For a slow speed it suffices to switch off the local battery, and the old system of working is resumed without touching anything else.

A wider range for adjusting the curb to produce any desired effect is accomplished by insulating the upper portion of the pillar at L. As all the curb signals are sent through R N I we can place in circuit at R resistance to reduce the curb signal, or extra cells to increase it.

On a long artificial cable this apparatus will be found to curb signals perfectly, and no defect will be found in the most difficult letters to curb, such as C F L K P Q R and Y. It will probably be found in practice that the usual receiving instrument does not show much improvement when the signals are curbed: this will be because the rate of vibration of the mirror or coil requires to be adjusted to the new condition of things. JNO. GOTT.



J. LEITCH & Co LITH. 13, 14 & 15 BEAR ALLEY, F

ON A UNION OR BINDING SCREW FOR TEMPORARY JOINT.

I venture to submit to the notice of the Society a small union or binding screw which I have devised for the purpose of temporarily joining leading wires together in the testing room. The annoyance of having to keep on screwing and unscrewing connections is thus avoided, and the ends of the wires are not crippled and nipped as in the ordinary binding screw.

The act of connecting the leading wire, especially if it be slightly twisted in the hand, appears to brighten and clean the end of the wire without requiring sand-paper, and the union appears good and firm.

There is a certain margin available in the diameter of the wire, but, for sizes varying much, different sized unions should be used.

It is made of a piece of brass wire simply hammered, drilled, and slotted, and I have had some in daily use for the last six months. A length of india-rubber tube slipped over it serves to insulate it and increase the power of the spring.

ARTHUR W. STIFFE,
Engineer and Electrician, Government Persian Gulf Telegraph.

Karachi, 17th January, 1878.

CORRESPONDENCE.

With reference to the paper on the Measurement of Currents inserted in the last number of the Proceedings, I find that the practice of recording the strength of currents in *Webers* has been in regular use for some years in the Military School at Chatham.

W. H. PREECE.

Feb. 23, 1873.

ABSTRACTS AND EXTRACTS.

From the PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY.
(Read June 19, 1873.)

I.—ON A STANDARD VOLTAIC BATTERY.

By LATIMER CLARK, M.I.C.E.

Communicated by SIR WILLIAM THOMSON, LL.D., F.R.S.

The object which the author had in view in pursuing the investigations alluded to in the following paper was to discover some form of voltaic battery which should have a perfectly constant electromotive force, and should maintain a uniform difference of electric potential between its poles. This want has been much felt by electricians, and the utility of such an investigation may be best shown by a brief reference to the recent history of electrical measurement.

In September, 1861, a paper was read by the author before the British Association for the Advancement of Science, advocating the adoption of a series of standard units of electrical measurement, and pointing out the mutual relations which should exist between such units. The subject was independently supported in Committee by Sir William Thomson, F.R.S., and the result was the appointment of a "Committee on Standards of Electrical Resistance," and a grant of money was set aside for the purposes of the Committee.

In 1862 the Committee presented their first Report; their numbers were then enlarged, further sums of money were voted for the continuation of their researches, and further Reports were presented in 1863, 1864, 1865, and 1867, after which the Committee was dissolved.

The Committee finally recommended the adoption of a system of natural electromagnetic units based on the metre and gramme,* in

* They have since adopted the centimetre-gramme unit.

which the unit current flowing through a conductor of unit length exerts the unit force on the unit pole at the unit distance. As these units were unfitted in magnitude for practical use, certain multiples have been adopted in practice, and have received names, and are now in almost universal use among electricians. These units are:—

1. Resistance.—The Ohm, equal to 10^7 absolute electro-magnetic metre-gramme units.

2. Capacity.—The Farad, equal to 10^{-7} absolute electro-magnetic units.

3. Potential.—The Volt, equal to 10^5 absolute electro-magnetic units.

The measure of quantity is the same as that of electrostatic capacity, and in practice generally receives the same name, although it has been sometimes called the "Weber;" the weber or farad quantity is equal to 10^{-2} absolute units. Electrical currents are defined as currents of so many farads per second. In this system the volt electromotive force through the ohm resistance produces the unit current, or a current of one farad per second.

The Committee determined with great care the value of the ohm resistance and the farad capacity, and issued standards which have been very extensively copied and distributed. They would naturally have desired to issue a standard of electromotive force, or degree of potential, and thus complete the series; but in this they met with insuperable difficulties, and finally separated without accomplishing this part of their task.

This was a matter of regret, seeing that the electromotive forces of batteries and the strength of currents are among the measures most frequently required by the practical electrician.

The difference of potential between two bodies may be measured by measuring the force of attraction between two electrified planes of known dimensions at a known distance, or two coils conveying currents. It may also be determined by similar means to those employed by the Committee in their determination of the absolute unit of resistance—that is, by revolving a coil at a known speed in a field of known magnetic intensity. If the value of the earth's horizontal magnetic intensity (H) were uniform at different times

and places, or easily obtained, and if the measurements were made at a distance from iron bodies, the tangent galvanometer would afford a means of absolutely measuring electromotive force.

All these methods, however, require complicated and expensive apparatus and great manipulative skill; and owing to these causes it may be safely asserted that not more than half a dozen absolute determinations of potential have ever yet been made. Practically, electricians have been compelled to define electromotive forces by comparison with those of the Grove's or Daniell's cell, the copper and zinc cell, or other electromotive sources; and it is a curious circumstance, that, among the thousand galvanic combinations known to exist, not one has been hitherto found which could be relied upon to give a definite electromotive force: however pure the materials, and however skilful the manipulation, differences varying from four or five per cent. upwards constantly occur without any assignable cause; and different observers using different materials of course meet with still larger discrepancies.

The author, sustained by a conviction that this difficulty could not, in the nature of things, be insuperable, has carried on a course of experiments since 1867 with a view to discover and obviate the cause of these variations, and has devised a form of battery which he desires to lay before the Royal Society, and which appears to meet, in a very satisfactory manner, all necessary requirements.

The battery is formed by employing pure mercury as the negative element, the mercury being covered by a paste made by boiling mercurous sulphate in a thoroughly saturated solution of zinc sulphate, the positive element consisting of pure distilled zinc resting on the paste.

The best method of forming this element is to dissolve pure zinc sulphate to saturation in boiling distilled water. When cool, the solution is poured off from the crystals and mixed to a thick paste with pure mercurous sulphate, which is again boiled to drive off any air; this paste is then poured on to the surface of the mercury, previously heated in a suitable glass cell; a piece of pure zinc is then suspended in the paste, and the vessel may be advantageously sealed up with melted paraffin wax. Contact with the mercury may be made by means of a platinum wire passing down a glass

tube, cemented to the inside of the cell, and dipping below the surface of the mercury, or more conveniently by a small external glass tube blown on to the cell, and opening into it close to the bottom. The mercurous sulphate ($Hg_2 SO_4$) can be obtained commercially;* but it may be prepared by dissolving pure mercury in excess in hot sulphuric acid at a temperature below the boiling-point; the salt, which is a nearly insoluble white powder, should be well washed in distilled water, and care should be taken to obtain it free from the mercuric sulphate (persulphate), the presence of which may be known by the mixture turning yellowish on the addition of water. The careful washing of the salt is a matter of essential importance, as the presence of any free acid, or of persulphate, produces a considerable change in the electromotive force of the cell.

The electromotive force of the elements thus formed is remarkably uniform and constant, provided the elements be not connected up and allowed to become weakened by working. A long series of comparisons was made between various elements, some of which had been made many months, and it was found that the greatest variation among them all did not differ from the mean value more than one-thousandth part of the whole electromotive force. Such a difference as this was however unusual, and might have been due to slight differences of temperature.

The following table gives some of the results obtained. Temperatures are not stated, as the elements were approximately at the same temperature at the standards with which they were compared at the time.

No. of element or letter.	Date of construction.	Date of comparison.	Value.
96	March 23, 1871	March 25, 1871	1·0000
16	February 16, 1871	March 24, 1871	·9997
89	March 23, 1871	March 25, 1871	·9991

* The author has obtained it from Messrs. Hopkins and Williams, 5, New Cavendish Street.

No. of element or letter.	Date of construction.	Date of comparison.	Value.
90	March 23, 1871	March 25, 1871	·9993
91	„	„	·9985
92	„	„	·9988
93	„	„	·9991
94	„	„	·9988
95	„	„	·9998
97	„	„	·9996
98	„	„	·9996
99	„	„	·9995
100	„	„	·9993
101	March 24, 1871	„	1·0006
102	„	„	1·0004
103	„	„	1·0003
104	„	„	1·0003
105	„	„	1·0001
106	„	„	·9995
115	March 27, 1871	March 28, 1871	1·0008
116	„	„	1·0005
117	„	„	1·0002
118	„	„	1·0005
119	„	„	1·0002
120	„	„	1·0001
A.	March 30, 1871	April 3, 1871	1·0003
C.	„	„	1·0003
E.	May 16, 1871	May 20, 1871	1·0005
F.	„	„	1·0005
D.	„	„	1·0003
L.	May 18, 1871	„	1·0004
155	December 1, 1871	December 19, 1871	1·0001
156	„	„	·9999
157	„	„	1·0001
158	„	„	1·0007
159	„	„	1·0003
160	February 17, 1872	February 26, 1872	1·0004

No. of element or letter.	Date of construction.	Date of comparison.	Value.
161	February 17, 1872	February 26, 1872	1·0004
162	„	„	1·0001
163	February 24, 1872	„	1·0002
164	„	„	·9999
165	„	„	1·0001
166	„	„	1·0001
167	February 28, 1872	February 29, 1872	1·0007
W. 1	September 11, 1872	October 9, 1872	·9999
2	„	„	1·0001
3	„	„	1·0003
4	„	„	·9996
5	„	„	·9996
6	„	„	1·0001
7	„	„	1·0003
8	„	„	·9999
9	„	„	·9997
10	„	„	1·0006
11	„	„	·9993
12	„	„	1·0004
13	„	„	·9993
14	„	„	·9994
15	„	„	1·0005
16	„	„	·9996
17	January 20, 1873	March 15, 1873	·9993
18	„	„	1·0001
19	„	„	1·0001
20	„	„	·9993
21	„	„	1·0004
22	„	„	1·0001
23	„	„	1·0005
24	„	„	1.0000
25	„	„	1·0005
26	„	„	1·0005
		Mean value . .	·9999

Several experiments were made to determine the variation of the electromotive force at different temperatures. From the mean of these it appears that the force decreases with increase of temperature in the ratio of about ·06 per cent. for each degree Cent.; for example, an element gave relative values of ·9993 at 0° Cent. and of ·9412 at 100° Cent. The element varies much more at temperatures near 0° Cent. than at temperatures near 100° Cent. The variation for about 10 degrees above or below 15·5 is ·06 per degree Cent. When the temperature is lowered from 15°·5 to 0° the force increases at the rate of ·08 per cent. per degree; when raised from about 15°·5 to 100° Cent. it diminishes at the rate of ·055 per cent. per degree.

The element maintains a sensibly constant electromotive force for one or two years, and possibly longer if the salts be prevented from drying by an air-tight covering.

It is not intended that this element should supersede any of the existing combinations in practical use for the production of a current; for it, like the Marie Davy and many other batteries, falls rapidly in electromotive force when allowed to work through a circuit of small resistance, though it recovers its original electromotive force if allowed to remain inactive for a short time. It is intended to be used chiefly as a standard of electromotive force with which other elements or sources of potential can be compared by means of an electrometer or of instruments (similar to the one described below) which do not require any current.

It will, however, continue to supply a permanent current through a circuit of large resistance, say 10,000 ohms, without any sensible diminution of its force, and has been advantageously applied to the testing of submarine cables.

The instrument used in comparing the element was one devised by the author in 1859 (see fig. 6, end of paper). The following diagram will explain its construction:—

a a represents a length of ten metres of platinum-iridium wire about ·5 millimetre diameter wound on a cylinder of ebonite, the ends being connected to the axes *b b′*, which work in blocks of metal with mercury contacts. Two batteries are also connected to the same blocks; the larger one, C, of several cells, sends a con-

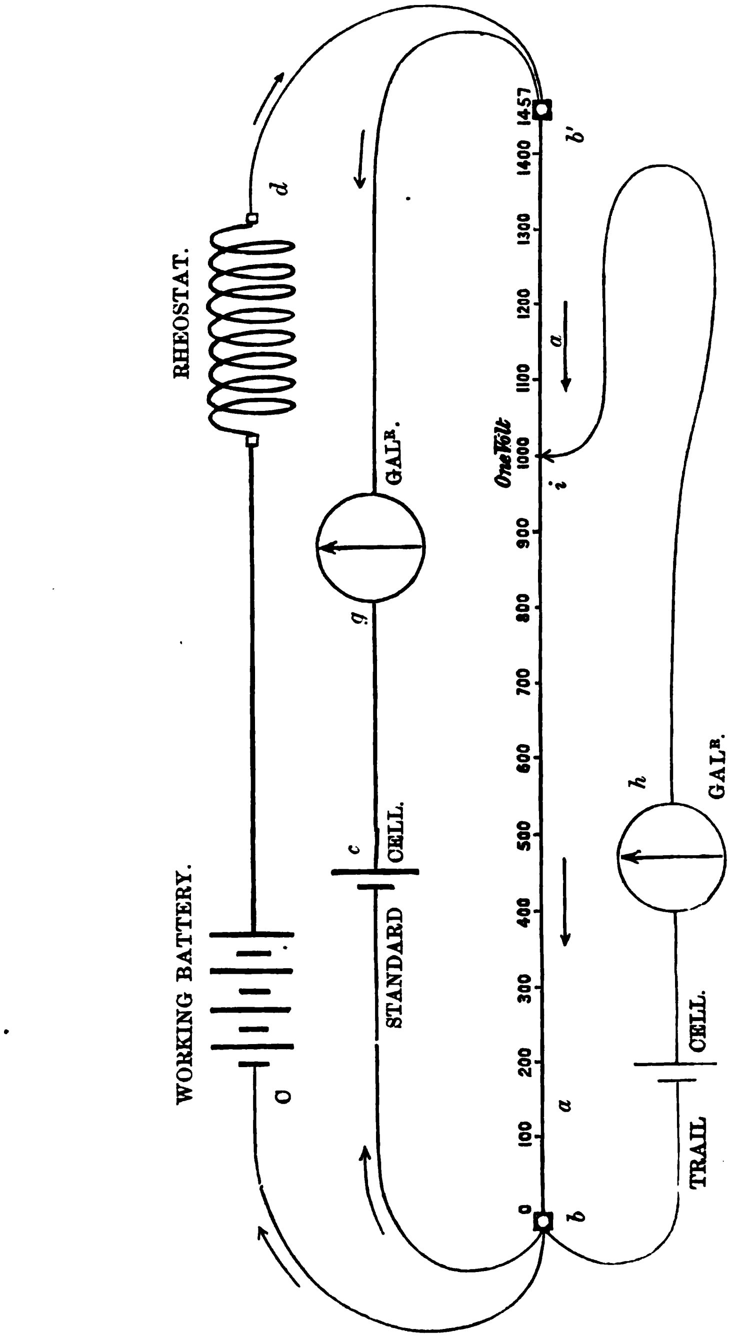

WORKING BATTERY.
RHEOSTAT.
STANDARD CELL.
GALR.
TRAIL CELL.
GALR.
One Volt
0
100
200
300
400
500
600
700
800
900
1000
1100
1200
1300
1400
1457
a
a
b
b′
c
d
g
h
i
o

Fig. 1.

tinuous current through the coil, the strength of which can be varied by means of the rheostat or resistance-coil *d*; the smaller, *c*, is the standard element; it is connected with the terminal blocks *b b'*, and it has a reflecting galvanometer, *g*, in circuit with it; as these two batteries are connected up in the same direction, they both tend to send a current through the coil *a a*. If the difference of potential maintained by the battery between the blocks *b b'* be greater than that of the standard cell, the battery will of course overpower the cell and send a reversed current through it; if, on the other hand, the difference of potential be less, then both the battery and the cell will jointly send a current through *a a*. In practice, however, the resistance, *d*, is so adjusted that the difference of potentials at *b* and *b'* is exactly the same as the difference of potential between the poles of the standard cell—in other words, equal to its electromotive force, in which case no current passes through the galvanometer *g*, and the cell remains inactive.

In comparing a trial cell with the standard, one pole of the cell is connected with that end of the coil to which the similar pole of the standard is fixed; the other pole is connected through a second galvanometer, *h*, to a sliding piece *i*. By means of this sliding piece contact can be made at any point of the coil *a a*, which is calibrated into 10,000 equal divisions. The point along the wire is readily found at which the potential is the same as that of the trial cell, and consequently no current passes through the galvanometer *h*; in this case the reading or number of divisions gives the value of the trial cell in ten-thousandth parts of the standard element. As it is necessary that the standard element should have a higher electromotive force than that which is compared with it, two or more cells may be employed as a standard.

Having thus obtained a constant and easily reproducible measure of electric potential, it became necessary to ascertain its pecise value in terms of the British Association units and in absolute measure. There are two well-known methods by which this may be accomplished; the one is by the use of Weber's electro-dynamometer,* and the other by means of the sine or tangent galva-

* Taylor's Scientific Memoirs, vol. iii.

nometer,* in which the force of the current, acting on a suspended needle through a known resistance, is compared with that of the earth's horizontal intensity. It was determined to measure the element by both methods.

The electro-dynamometer employed was an instrument constructed for the British Association Committee, and referred to in their Report for 1867, page 478. This instrument had not been previously used.

In the electromagnetic system, the unit length of the unit current, acting on another similar current at the unit distance, exercises the unit of attractive or repulsive force. The value of the current (C) in absolute units may therefore thus be determined from its mechanical effect; and, the resistance (R) of the circuit being known, the value of the electromotive force (E) follows from Ohm's formula,

$$C = \frac{E}{R} \text{ or } E = CR.$$

In the instrument in question (fig. 2) the large fixed coil is double, as in the arrangement given by Helmholtz and Gaugain to the tangent galvanometer, the two coils being in parallel vertical planes at a distant apart equal to their radius; the small coil is also double, and is suspended bifilarly truly central to the fixed coils, the bifilar suspension-wires being used to convey the current between the fixed and movable coils.

The top of the instrument (fig. 3) is furnished with various contrivances for facilitating the central adjustment of the coils; these consist of two plates, forming a slide-rest movement fitted with verniers, by which horizontal motion can be given to the suspension in any direction. The upper plate carries a circular collar, which can be rotated by a tangent screw, and is graduated to 360 degrees. Into this collar fits a brass frame, carrying two ebonite blocks, on which are two horizontal sliding pieces, diametrically opposite to one another, each furnished with a vernier, and terminating interiorly in small brass pulleys or rollers, against which the suspension-wires rest, and by which their distance apart can be regulated.

* British Association Report, 1863, pp. 116, 141.

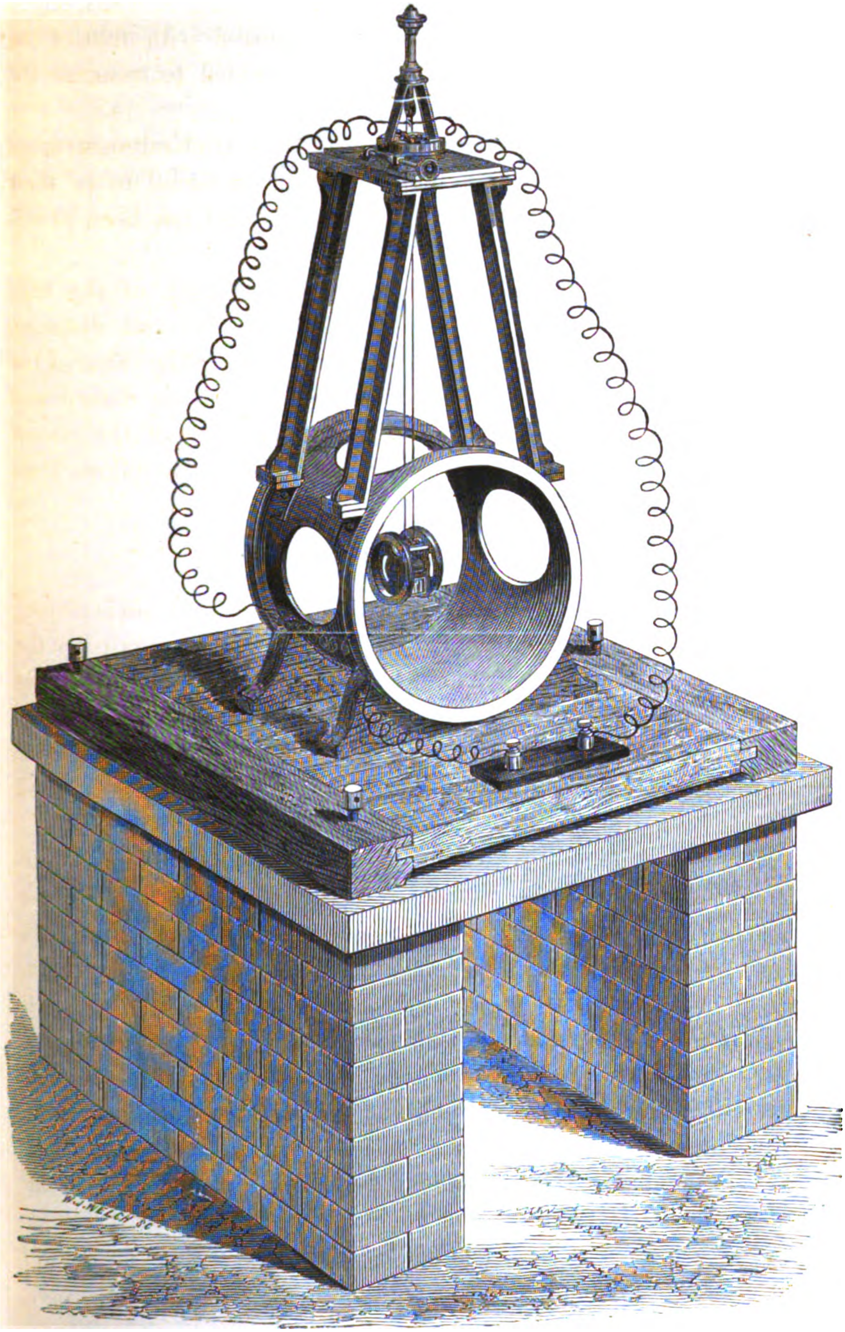


Fig. 2.

The frame carries a light pulley, three centimetres diameter, which supports the suspension-wires by means of a silk cord passing over the pulley and attached to the wires near the top, so as to insure an equality of tension on the two wires. The wires pass down through the collar and socket to the lower coil. The suspension-pulley admits of upward and downward adjustment by means of a milled head screw.

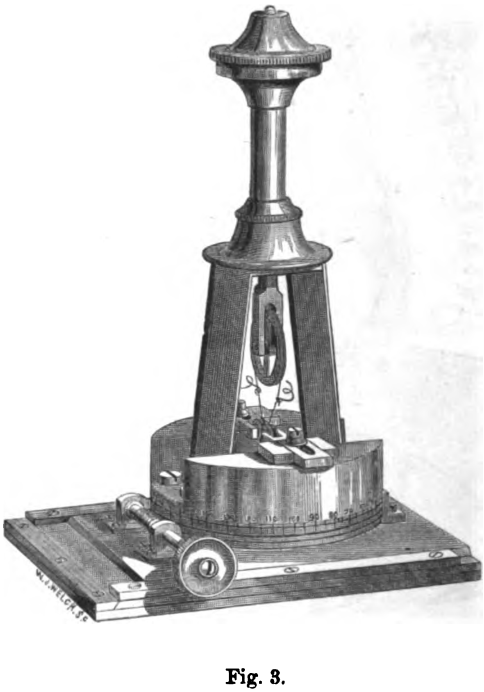


Fig. 3.

The electro-dynamometer with its telescope and stand was supported on a solid brick foundation; the scale was a metre long, divided into millimetres, and was fixed at a distance of 2·7 metres from the centre of suspension. The scale was carefully adjusted at right angles to the axis of the telescope. A plate of silvered glass was fixed at the back of the large coils and adjusted parallel to them, and upon it was marked the centre of the coil accurately determined. When this centre mark, viewed through the telescope,

was brought to coincide with the cross wires by moving the large coils, and when the centre of the telescope reflected in the silvered glass also coincided with the cross wires, the telescope was of course directed normally to the centre of the plane of the coils, which were adjusted in the magnetic meridian; excessive care was taken with all the adjustments and readings, which were repeated with reversed currents and positions.

In order to maintain a current through the coils of the dynamometer, and to ensure that the difference of potential between its poles should be precisely equal to that of the standard cell, an

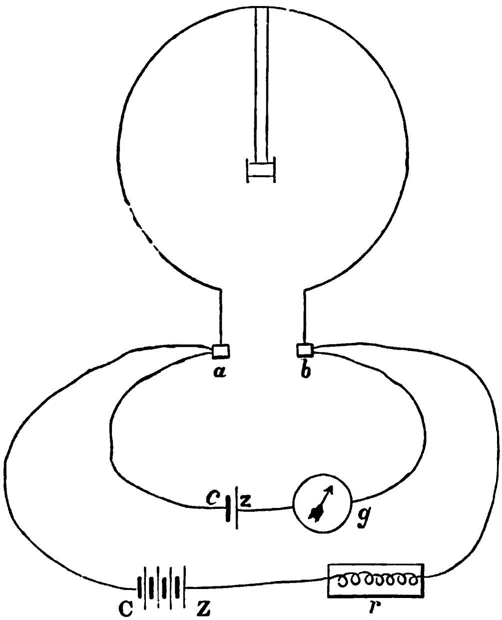


Fig. 4.

auxiliary battery was used in the manner before described. This consisted of five large Daniell's cells working through a circuit consisting of the dynamometer and a rheostat, *r* (fig. 4); *a* and *b* are the two terminals of the dynamometer, and the poles of the auxiliary battery, C, Z, are connected with them; the similar poles of the standard elements *c*, *z*, and the galvanometer, *g*, are also connected to the same terminals; and the rheostat *r* is adjusted

so that no current passes through the galvanometer. In this case it is evident that the poles a and b are maintained at a difference of potential precisely equal to that of the standard elements. In this arrangement not the slightest difficulty is experienced in maintaining a perfectly steady and uniform current through the coils of the dynamometer. The poles of the dynamometer were so arranged that they could be connected immediately with a Wheatstone balance in order that its resistance could be measured promptly after each observation.

The winding of the large coils of the instrument was superintended by Professor Clerk Maxwell, who kindly supplied me with the measurements as follows :—

	Millimetres.
Mean circumference of first coil	1558·48
Mean circumference of second coil	1559·16
The depth of each coil is	12·90
The breadth of each coil is	15·00
The distance apart of the planes of the coils .	250

Each layer has 15 windings, and there are 15 layers, so that each coil has 225 windings.

The small coils were wound afresh by myself; the brass channels for the reception of the wire were of different sizes, and the same number of turns could not therefore be wound on it.

	Millimetres.
Mean circumference of first coil	359·25
Mean circumference of second coil	357·45
Mean depth of the coils	6·67
Mean breadth of the coils	10·52
Mean distance apart	62·41
Number of windings on first coil	311
Number of windings on second coil	327

The moment of inertia of the suspended coil was determined from a great number of observations by different methods.

(1) The coil was vibrated on a fine steel wire, and the moment was then increased by a gun-metal cylinder passing through the

centre of the coil; the increased time of vibration was then observed, and the moment of the coil calculated by the formula

$$I = \left(W\frac{l^2}{12} + \frac{d^2}{16}\right)\frac{t^2}{t'^2 - t^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where W is the weight of the cylinder in grammes, l and d its length and diameter in metres, and t' and t the times of vibration of cylinder and coil and of coil.

(2) From the value so ascertained the dimensions of a gun-metal cylinder were calculated, having about the same moment of inertia as the coil when vibrating on a transverse diameter: two of these rings were accurately formed by Mr. Becker and carefully weighed; their times of vibration were compared with that of the coil when suspended from the same wire. The corrected moment of the coil was then calculated from these times by the formula

$$I = W\frac{t^2}{t'^2}\left(\frac{r^2 + r'^2}{4} + \frac{a^2}{3}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where t and t' are the times of a vibration of the coil and the ring, r and r' the external and internal radii of the ring, $2a$ its breadth, and W its weight.

(3) The moment of inertia was also determined by the vibration about its longitudinal axis of a metal cylinder of small thickness compared with its radius, as suggested by Sir William Thomson, F.R.S. (Proc. Royal Society, vol. xiv. p. 294), the coil and cylinder being alternately vibrated on the same wire.

The following were the results of the observations :—

First system, mean of five observations .	1·27641
Second system, mean of twenty observations	1·27680
Third system, mean of one observation .	1·27795
Moment of inertia employed in calculation	1·27691 metre-gramme.

It is not necessary to give the mathematical formula used in calculating the values of E, but the following table gives the results of the whole of the series of observations with the electro-dynamometer.

Date.	Value of E in volts.	Remarks.
8 December 1871 .	1·4585	3 cells.
9 " "	1·4651	3 cells.
14 " "	1·4616	3 cells.
15 " "	1·4561	3 cells.
15 " "	1·4579	2 cells.
16 " "	1·4586	3 cells.
16 " "	1·4517	3 cells, coil turned 180°.
16 " "	1·4552	2 cells, coil turned back 180°.
16 " "	1·4565	3 cells.
16 " "	1·4535	2 cells.
16 " "	1·4564	3 cells.
18 " "	1·4649	3 cells.
19 " "	1·4562	3 cells, coil turned 180°.
19 " "	1·4558	3 cells, coil turned back 180°.
20 " "	1·4615	3 cells.
20 " "	1·4539	3 cells.
20 " "	1·4551	2 cells.
21 " "	1·4549	3 cells.
Mean value of E .	1·45735 volt.	Temperature 15°·5 Cent.

The cells were frequently changed during the course of the experiments. Values were also obtained when the suspended coil was moved two millims. in various directions about the centre, but they did not differ sensibly from the above.

As a verification of the results obtained with the electro-dynamometer, the electromotive force of the new element was also determined by means of the sine galvanometer by a method which is well known, viz.:—

$$E=\left(\frac{K}{2\pi n}H\sin\theta\right)\times R, \quad . \quad . \quad . \quad . \quad (3)$$

where E is the electromotive force, K is the radius of the circle, n the number of turns, H the horizontal intensity of the earth's magnetism, θ the angle through which the coils must be turned in order to maintain the needle in the plane of the coils, and R the resistance of the circuit in absolute measure.

The instrument employed was specially constructed for these experiments, and presents some novelties. The needle was one

centimetre in length, and was furnished with a mirror of parallel glass silvered by the chemical process, so that the reflection from either side could be observed in the telescope. The coil was 140 millimetres in diameter, and was furnished with a large mirror accurately parallel to its plane, silvered on the observing or front side, and having the centre of the coil marked upon it; by the aid of the telescope and these mirrors it was easy to adjust the needle accurately to the centre of the coil, and to insure that the plane of the coil was truly vertical, and coincided with the magnetic meridian.

The telescope was carried on an arm one metre in length, which with the coil turned on a pair of theodolite plates; and thus readings could be taken to half minutes. The experiments were performed within five miles of the Royal Observatory. The value of H, a knowledge of which is necessary for the determinations with this instrument, was kindly supplied to me by the Astronomer-Royal for each day on which the observations were taken. No iron was near the instrument.

A difference of potential equal to that of one standard cell was maintained between the poles of the sine galvanometer by the use of an auxiliary battery, rheostat, and galvanometer, in the manner described when treating of the dynamometer observations.

The following table gives the results of these experiments :—

Date.	Value of H.	Value of E.	Remarks.
9 Feb.	1·788	1·45605	Galvanometer wound with 8 turns German silver wire.
9 "	"	1·45457	
9 "	"	1·45400	
10 "	1·788	1·45809	
10 "	"	1·45669	
11 "	1·788	1·45799	
18 "	1·787	1·45566	Re-wound with 28 turns German silver wire.
19 "	"	1·45671	
19 "	"	1·45680	Re-wound with 27 turns German silver wire.
20 "	1·787	1·45752	
20 "	"	1·45645	
24 "	1·786	1·45522	
24 "	"	1·45492	
Mean value of E . .		1·4562 volt.	Temperature 15°·5 Cent.

The observations are corrected for the temperature of the element and of the coils; but the correction for the breadth and depth of the coil, according to Professor Clerk Maxwell's formula,* was so small as only to appear in the fifth place of decimals, and was therefore neglected. The instrument was re-wound twice with various lengths of wire.

We have therefore the mean value of the electromotive force of the standard cell:—

	Volt.
1. As determined by the electro-dynamometer (18 observations)	1·45735
2. As determined by the sine galvanometer (13 observations)	1·45621
Mean value of E. . . .	1·45678

or, since no importance can be attached to the figures beyond the third place of decimals, 1·457 volt, equal to 145700 absolute electromagnetic units.

The uses of this standard element to practical electricians are sufficiently obvious. It may be used for determining the electromotive force of other elements by the use of an electrometer or by the discharge from a condenser. Or a condenser having a capacity of $\frac{1}{1\cdot457}$ farad charged by the standard cell would contain the B. A. unit quantity of electricity (one Weber), or $\frac{1}{100}$ of the absolute unit of quantity.

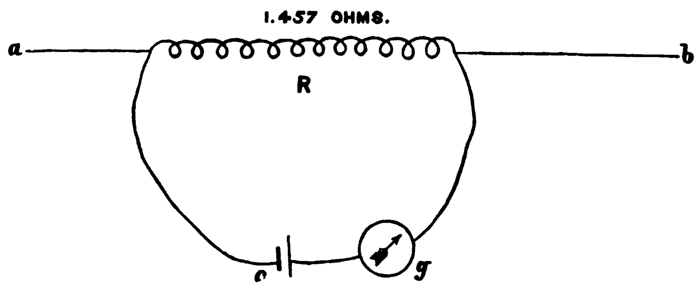


Fig. 5.

It is also of great value for maintaining a current of known

* British Association Report, 1863, p. 170.

strength in any circuit for the purposes of experimental research. Thus if it be desired to produce in any circuit (*a b*, fig. 5) a current equal to the B. A. unit of current ($\frac{1}{100}$ absolute units), it is only necessary to insert in the circuit a wire R having a resistance of 1·457 ohm, and to connect to each end of this wire the poles of a standard cell, *c*, with a galvanometer, *g*, and to vary the strength of the current in *a b* until no deflection is produced on the galvanometer; the current through *a b* will then be equal to one B. A. unit of current, or 1 farad per second, whatever its length or resistance.

By varying the resistance of R, or by varying the number of elements *c*, any given current can be steadily maintained through *a b* at pleasure; on the other hand, the value of any given current can be measured by so varying the resistance R that no deflection is produced on the galvanometer. The value of the passing current will then be

$$C = \frac{1 \cdot 457}{R} \text{ farad per second.}$$

It is also evident that, knowing the value of E, we may determine the horizontal intensity of the earth's magnetism, H, in any place quickly and simply by means of an ordinary sine or tangent galvanometer.

Thus by transposing the equation (3) we have for the tangent galvanometer

$$H = \frac{E\, 2\pi n}{R K \tan\theta} \quad \ldots\ldots\ldots \quad (4)$$

In fact the standard of electric potential is second only in importance to that of the standard of electric resistance; and the use of such a standard, combined with an auxiliary battery in the manner described in the foregoing paper, admits of a variety of applications which it is believed will be found of great value in electrical research.

In conclusion I have to acknowledge the great assistance I have received from Mr. Herbert Taylor, C.E., and Dr. Alexander Muirhead, in conducting these experiments.

ON THE GENERAL THEORY OF DUPLEX TELEGRAPHY.

By Louis Schwendler.

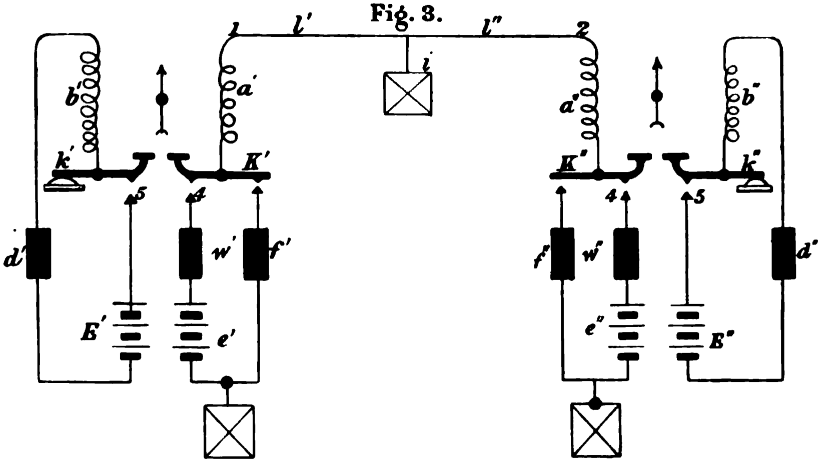
Paper read before the Asiatic Society of Bengal.

(Continued from p. 543, vol. vi.)

III. The compensation method.*

This method is the oldest. Fig. 3 gives the general diagram.

Fig. 3.



Explanation of Diagram.

e is the E. M. F. of the line battery.

β its internal resistance.

E is the E. M. F. of the compensation battery.

α its internal resistance.

K is a constant resistance key. Dr. Gintl used an ordinary key, which, it will be obvious, must result in a failure.

---

* Dr. Wilhelm Gintl, *k*, *k* Director-General of Telegraphs in Austria, is the inventor of this earliest method. In 1853 he made the first practical experiment on a line between Vienna and Prague (240 miles).

k is an ordinary key; both keys, in the same station, are worked simultaneously, *i. e.*, contacts 4 and 5 are closed and broken at one and the same time.

d, *f*, and *w* are certain resistances.

a is the *one* coil of the differential instrument which is connected up in the line circuit.

b is the *other* coil of the differential instrument which is connected up in the compensation circuit. By *a* and *b* shall be also designated the resistances of these two coils.

The coils *a* and *b* with their batteries *e* and *E* respectively are arranged in such a manner that they have opposite magnetic effects with respect to the same magnetic pole acted upon. The two circuits in each station (the line circuit and the compensation circuit) are insulated from each other. All the other terms, as L, L', L'', &c. shall have the same physical meaning as before.

The compensation method has two principal defects which the two preceding methods do not possess.

Firstly. The success of working a line dupliciter by the compensation method will clearly depend on the possibility of being able to close and open simultaneously two different contacts (4 and 5). The mechanical difficulty of doing so sufficiently accurately was pointed out by Dr. Werner Siemens, and in fact constitutes one of the reasons which led him to propose the differential method.

Secondly. The balance in each station may be disturbed *directly* by a variation of the electrical condition (internal resistance and E. M. F.) of the two batteries (*E* and *e*) employed.

In the preceding two methods the variation of the internal resistance of the signalling battery can only be felt *indirectly* by affecting the balance of the distant station, while the variation of E. M. F. has no effect at all. Hence a given variation in the battery or batteries must necessarily produce a greater disturbance of balance in the *compensation method* than in the two preceding ones. We know that even so-called constant galvanic batteries, doing work, alter their electrical conditions perceptibly, especially their internal resistance, and consequently this defect weighs most decidedly against the compensation method. In all other respects the compensation method has the same defects as the differential method, and in addition some others which will be understood as the investigation proceeds.

General expressions for the two functions "D" and "S."

To obtain the functions D and S we have to develope the general expressions for the forces p, P, and Q, say for Station I.

$$p' = A' \, m' - B' \, n'$$

where A' and B' are the currents which pass through the two coils a' and b' respectively, when Station I. is sending and Station II. is at rest; m' and n' are the forces exerted by the two coils a' and b' respectively on one and the same magnetic pole when a unit of current passes through them. At balance in Station I. $p' = o$

Further $$P' = \mathfrak{A}' \, m'$$

where $\mathfrak{A}'$ is the current which passes through the coil a' when Station II. is sending and Station I. is at rest (single signals).

Further $$Q' = \mathfrak{V}' \, m' + \mathfrak{B}' \, n'$$

where $\mathfrak{V}'$ and $\mathfrak{B}'$ are the currents which pass through the coils a' and b' respectively when both stations are sending simultaneously (duplex signals).

The compensation circuit and the line circuit in each station being electrically independent of each other we have

$$\mathfrak{B}' = B'$$

invariably without condition.

If we further presuppose that depressing of the key K does not alter the complex resistance of the station, a condition which, for the regularity of signals, we are obliged to assume here as well as in the two preceding methods, it will be clear that

$$\mathfrak{V}' = A' + \mathfrak{A}'$$

Substituting these values for $\mathfrak{V}'$ and $\mathfrak{B}'$ in the expression for Q', we get:

$$\begin{aligned} p' &= A' \, m' - B' \, n' \\ P' &= \mathfrak{A}' \, m' \\ Q' &= (A' + \mathfrak{A}') \, m' + B' \, n' \end{aligned}$$

The signs of the terms may be again contained in the currents, while m' and n' are taken as absolute numbers. We must only remember that $A' \, m'$ and $B' \, n'$ must be invariably of opposite sign.

Arbitrarily we will call the current A positive when the negative pole of the line battery is to earth.

Now we have again two different modes of connecting up the line batteries, viz.:—

1*st.* The same poles of the line batteries are connected to earth in the two stations:

$$\begin{aligned} p' &= \pm A' m' \mp B' n' \\ P' &= \mp \mathfrak{A}' m' \\ Q' &= (\pm A' \mp \mathfrak{A}') m' \mp B' n' \end{aligned}$$

2nd. Opposite poles of the two line batteries are connected to earth in the two stations:

$$\begin{aligned} p' &= \pm A' m' \mp B' n' \\ P' &= \pm \mathfrak{A}' m' \\ Q' &= (\pm A' \pm \mathfrak{A}') m' \mp B' n' \end{aligned}$$

Subtracting in either case P' from Q', we get

$$Q' - P' = S = p'$$

Or, on account of having fulfilled the key equation $f = w + \beta$, the difference of the forces which produce single and duplex signals is equal in sign and magnitude to the force by which balance is disturbed. Further it is, also for the compensation method, quite immaterial whether the same or opposite poles of the two line batteries are connected to earth. As pointed out, it is preferred to connect the same poles, *i.e.*, the negative poles of the line batteries to earth.

Assuming this case we have:

$$\begin{aligned} p' &= A' m' - B' n' \\ P' &= - \mathfrak{A}' m' \\ Q' &= (A' - \mathfrak{A}') m' - B' n' \end{aligned}$$

Substituting now for A', B', and $\mathfrak{A}'$ their values, and remembering that

$$\left. \begin{aligned} m' &= q' \sqrt{a'} \\ n' &= r' \sqrt{b'} \end{aligned} \right\} \text{approximately,}$$

we get the following general expressions for the two functions D and S:

$$\left.\begin{aligned} S' &= e'\, q'\, \frac{\Delta'}{R'\, K'} \\ D' &= \frac{e'}{e''} \cdot \frac{K''}{R'\, K'} \cdot \frac{\Delta'}{\mu' \sqrt{a'}} \end{aligned}\right\} \text{for Station I.}$$

and
$$\left.\begin{aligned} S'' &= e''\, q''\, \frac{\Delta''}{R''\, K''} \\ D'' &= \frac{e''}{e'} \cdot \frac{K'}{R''\, K''} \cdot \frac{\Delta''}{\mu'' \sqrt{a''}} \end{aligned}\right\} \text{for Station II.}$$

where
$$\begin{aligned} \Delta &= R \sqrt{a} - K \lambda v \sqrt{b} \\ R &= a + b + d \\ K &= f + a + c \\ \lambda &= \frac{E}{e} \\ v &= \frac{r}{q} \end{aligned}$$

Rigid fulfilment of the two functions S = o *and* D = o.

For finite quantities these two functions can only become zero if $\Delta = o$, *i. e.*,

$$R \sqrt{a} - K \lambda v \sqrt{b} = o$$

which is the balance equation for the *compensation method.*

To fulfil this equation permanently, no matter what the special cause of disturbance may be, we can again adopt two essentially different modes of re-adjustment, viz.:—

Either leave the two coils a and b or their armatures stationary, and adjust balance by altering the resistance in one or both of the two circuits, or leave the resistances constant and alter the relative position of the two coils or their armatures with respect to a given magnetic pole. These two methods of re-adjusting balance shall be considered separately.

a. Re-adjustment of balance by altering resistances.

In order to have *immediate balance* it will be clear that the alteration of resistance must be restricted to the compensation circuit, which is electrically independent of the line circuit. The total resistance in the compensation circuit consists of three different resistances, namely b, a, and d. Neither b nor a, consider-

ing their nature, can conveniently be made adjustable in practice; hence the alteration of resistance in the compensation circuit is restricted to d, which must therefore consist of increments of the proper size. The adjustment of d should be quick and convenient.

In addition to this adjustment, $\lambda = \frac{E}{e}$ may be made adjustable by varying E in increments of *one* cell. Such an adjustment is however not fine enough for ordinary use. The E. M. F. of one cell is too large a quantity in comparison with the total E. M. F. used in the compensation circuit. If the variation of the line current becomes very great it might perhaps be found convenient to alter E, but as an ordinary mode of adjustment it must be dispensed with.*

It is scarcely needed to point out that to adjust balance by altering the line current, either by varying the resistance or the E. M. F.† or both, of the line circuit, must be rejected once for all, because such an adjustment of balance in the one station could never take place without disturbing the balance of the other station; or in other words the required *immediate balance* could not be fulfilled.

b. Re-adjustment of balance by moving the coils or armatures.

If we suppose both the coils or their armatures simultaneously moveable in the same direction, then clearly this mode of adjustment contains not only the required immediate balance but in addition represents also a very rapid and entirely continuous action. For this reason it is apparently preferable to the first method, where the adjustment can only be carried on in *one* branch by varying d in increments.‡ Which of the two methods, how-

* During the period of low insulation of the line it might be advisable and practicable to make E larger than during the period of high insulation of the line (wet and dry season).

† Alteration of E. M. F. of a galvanic battery cannot be achieved without altering its internal resistance. Hence varying e would also involve a variation of β, and in order to keep $f = w + \beta$, it would become necessary to alter w simultaneously with e, i. e., w would have to be increased when e decreases and vice versa. This method, besides being rough, would therefore be also inconvenient.

‡ It has been suggested to adjust balance by a continuous variation of resistance,

ever, is to be chosen finally, depends on other considerations which will become clear further on. We know now that both these modes of adjustment are convenient and practicable, and contain *immediate* balance without special conditions. In fact in this respect the compensation method is preferable to the differential method where immediate balance by varying resistances could only be obtained when varying the four branches simultaneously according to a fixed relation.

Rapid approximation of the two functions "S" *and* "D" *towards zero.*

On account of $f = w + \beta$ we have

$$S = p = e\, q\, \frac{\Delta}{R\, K}$$

where $$\Delta = R \sqrt{a} - K \lambda v \sqrt{b}$$

as for instance by moving a contact point along a thin platinum wire in the same manner as Dr. Werner Siemens has done it in his bridge employed for comparing accurately comparatively small resistances. It is however scarcely necessary to point out that such a method, if applied for duplex working, must result in a failure, at all events so long as electro-magnetic instruments are used for producing the signals. For in such a case the resistance of any branch, no matter what special duplex method may be employed, must bear a certain ratio to the given resistance of the line, in order to get the signals with sufficient force. This ratio, as my investigations have shown, is by no means a small one, and hence the resistances of all branches, even for a short line, cannot be made small. Therefore the platinum wire, constituting one or two branches of the duplex method employed, must also offer a considerable resistance, *i.e.*, must be of great length. Hence to alter such a large resistance continuously and perceptibly, as is indicated by the balance disturbance, must evidently involve a considerable movement of the contact point, which, even choosing the thinnest possible wire, and the shortest telegraph line, becomes already for the daily variation so large as to make its application impossible. Unless another material of much higher specific resistance than platinum wire can be found, which at the same time allows of the sliding contact being made securely, the adjustment of balance by a continuous variation of resistances must be dispensed with. Such a material does not appear to exist. I thought of acting on Phillips's suggestion to use pencil-marks for the adjustable resistance, and although I found that pencil-resistances can be adjusted very accurately, and can be enclosed in a very small space, and that they keep sufficiently constant, it is difficult, if not impossible, to alter them by a sliding contact. The "*Uebergangs-widerstand*" is too variable and too great. Besides if the contact is made with sufficient pressure its sliding along alters the thickness of the pencil mark, and hence the resistances become inconstant and uncertain.

Now suppose $\Delta = o$, then this equation may be disturbed by K, R, λ, v, a, or b varying; a and b are wire resistances which may be taken as constant, for their variation with temperature is exceedingly small, and in case of accident, *i. e.*, a coil breaking or becoming shunted, nothing short of actual repair could help. Further v, supposing the differential instrument to be properly designed and mechanically well executed, may be taken as a perfectly constant quantity, which certainly, as long as the coils or their armatures are not moved on purpose, does not alter of its own accord.

The quantities left, which by variation may affect the balance equation, are K, R, and λ.

Of these three quantities the variation of K may become largest, for K does not only contain the line resistance, which is highly variable, but K includes also the internal resistance of both the line batteries, which, even for the best known form of galvanic battery, is by no means a constant quantity. The variation of the internal resistance of the line battery in each station produces of course the greatest disturbance of balance in *that* station.

The next quantity most liable to change of its own accord is clearly R, since it contains the internal resistance of the compensation battery.

λ, the ratio of the two E. M. F's. in one and the same station, though being also liable to change, will however vary very little. The E. M. F. of a well-prepared galvanic battery, especially when the battery is worked by weak currents, is far more constant than is generally believed.*

With respect to the variation of the three quantities K, R, and λ, the function S may therefore be expressed in three different forms.

* It appears that changes which have been observed to take place in the E. M. F. of a Minotto or Leclanché's battery are generally apparent only, not real. Such changes are generally quite within the limits of observation errors, and if they are large they are then generally due to the incorrectness of the method employed for measuring the E. M. F., or to cells actually having become exhausted. It appears that this mysterious force in each cell either exists in its full vigour, or not at all; there seems to be no continuous change in either direction.

$$S_1 = e\,q \frac{\lambda\, v\, \sqrt{b}}{R\, K}\, \delta\, K \text{ when } K \text{ varies only.}$$

$$S_2 = e\,q \frac{\sqrt{a}}{R\, K}\, \delta\, R \text{ when } R, \textit{ i. e., } a, \text{ varies only.}$$

$$S_3 = e\,q \frac{v\, \sqrt{b}}{R}\, \delta\, \lambda \text{ when } \lambda, \textit{ i. e., } E \text{ or } e, \text{ or both, are}$$

varying only.

These three different disturbances of balance may act singly or conjointly, and it is clear that they are independent of each other, at all events as far as this investigation is concerned. Consequently the safest plan will be to make each influence as small as the circumstances will allow it.

The disturbance S_1 for any constant $e\, q\, \lambda\, v\, \sqrt{b}$, and any given $\delta\, K$, will obviously become smallest the larger $R\, K$ is selected. Supposing $R + K$ constant, whatever that value finally may be, $R\, K$ has a maximum for $R = K$, and the very same condition will obviously make the disturbance S_2 smallest.

S_3 offers no best condition; this expression only shews that it has an absolute maximum with respect to b, namely as

$$R = a + d + b, \text{ for } b = a + d.$$

Thus we are informed that whatever relation between b and $a + d$ may be finally chosen, $b = a + d$ should *not* be selected, as otherwise any given variation of λ would have the greatest possible disturbing effect on the balance. But $b = a + d$ being the condition for the maximum magnetic effect in the compensation circuit, it is hereby established that for the sake of regularity of signals, which under all circumstances is to be considered of paramount importance in duplex telegraphy, the magnetic effect in the compensation branch *must not* be achieved in the most economical manner, but quite the reverse. This, as the compensation circuit has actually to produce wholly or partly the *duplex signals*, is a testimoniam paupertatis for the compensation method, and proves it in this respect inferior to both the *double balance* and *the differential method.*

$$R = K$$

is the regularity condition for the compensation method, *i. e.*,

*In order to make the disturbance of balance by a variation of the resistance in both the circuits absolutely as small as possible, the total resistance of the compensation circuit should be equal to the total resistance of the line circuit.**

If we now substitute in S_1 for K the value R, and in S_2 for R the value K we get

$$S_1 = e\,q\,\frac{\lambda\, v \sqrt{b}}{R^2}\,\delta K$$

$$S_2 = e\,q\,\frac{\sqrt{a}}{K^2}\,\delta R$$

while $$S_3 = e\,q\,\frac{v \sqrt{b}}{R}\,\delta\lambda \text{ remains the same.}$$

S_1 has an absolute maximum for $b = \frac{a + d}{3}$; S_2 for $a = \frac{f + c}{3}$; and S_3 for $b = a + d$ as stated before.

Hence we know what relations between the different variable should *not* exist.

This is all we can get from the function S. For further relations we must look to the function D.

For Station I. we have †

$$D' = \frac{e'}{e''}\,\frac{K''}{R'\,K'}\cdot\frac{\Delta'}{\mu' \sqrt{a'}}$$

which again, with respect to the variations of K', R', and λ', may be written in three different forms :

$$D_1' = \frac{e'}{e''}\;\frac{K''}{R'\,K'}\cdot\frac{\lambda'\, v' \sqrt{b'}}{\mu' \sqrt{a'}}\;\delta K'$$

$$D_2' = \frac{e'}{e''}\cdot\frac{K''}{R'\,K'}\cdot\frac{1}{\mu'}\,\delta R'$$

* This result is against the adopted view, for Dr. Gintl as well as others after him have always treated the compensation circuit as a kind of *local circuit*, *i. e.*, giving to it as low a resistance as practice allows. But this is clearly wrong, for if R is made very small as compared with K the balance becomes unstable. This fact explains, to a certain degree, the failure which has attended the application of the compensation method for duplex working, because the method was tried under the most unfavourable quantitative arrangements.

† When investigating the minimum absolute magnitude of S, the terms could be

and $$D_3' = \frac{e'}{e''} \cdot \frac{K''}{R'} \cdot \frac{v'}{\mu'} \frac{\sqrt{b'}}{\sqrt{a'}} \delta \lambda'$$

Considering that

$$\frac{K''}{K'} = \frac{i + l'' + \rho''}{i + l' + \rho'}$$

$$\mu' = \frac{i}{i + l' + \rho'}$$

and $$\frac{K''}{\mu'} = L + \rho' + \rho'' + \frac{(l' + \rho')(l'' + \rho'')}{i}$$

we have $$D_1' = \frac{e'}{e''} \cdot \frac{i + l'' + \rho''}{i} \qquad \frac{\lambda' v' \sqrt{b'}}{R' \sqrt{a'}} \cdot \delta K'$$

$$D_2' = \frac{e'}{e''} \cdot \frac{i + l'' + \rho''}{i} \cdot \frac{1}{R'} \delta R'$$

$$D_3' = \frac{e'}{e''} \left\{ L + \rho' + \rho'' + \frac{(l' + \rho')(l'' + \rho'')}{i} \right\} \frac{v'}{R'} \frac{\sqrt{b'}}{\sqrt{a'}} \delta \lambda'$$

put $$\frac{e'}{e''} = s$$

$$\frac{i + l'' + \rho''}{i} = J$$

$$L + \rho' + \rho'' + \frac{(l' + \rho')(l'' + \rho'')}{i} = T$$

and $$\frac{1}{R' \sqrt{\frac{a'}{b'}}} = \frac{1}{\psi'}$$

$$\therefore \quad D_1' = sJ\lambda' v' . \frac{1}{\psi'} \delta K'$$

$$D_2' = sJ \frac{1}{R'} \delta R'$$

$$D_3' = sv' \frac{T}{\psi'} \delta \lambda'$$

Now keeping s, J, λ', and v' constant, D_1' becomes smallest for any given $\delta K'$ the larger ψ' is selected; while D_3' becomes smallest for any given $\delta \lambda'$ the smaller $\frac{T}{\psi'}$ is selected, and D_2' becomes smallest the larger R' is chosen.

taken without an accent, because S contains only terms belonging to the same station. When investigating D this cannot be done, as D contains also terms belonging to the other station.

Now $\psi' = R' \sqrt{\frac{a'}{b'}}$ has a maximum for $a' = b'$; for $R' = b' + a' + d' = b' + \gamma'$, and putting $\gamma' = b' t'$ we have $\psi' = (1 + t') \sqrt{a' b'}$ which for $a' + b'$, and t' constant, has clearly a maximum for $a' = b'$. This proceeding is right, because we take b' as the original variable, and vary a' and γ' simultaneously with b', in order to keep t' and $a' + b'$ constant; while J and s are independent of a', b', and γ'.

In order to be sure that $a' = b'$ makes also D_3' a minimum, we must shew that T keeps constant, *i.e.*, ρ' keeps constant when a' varies. But $\rho' = a' + f'$, thus we have only to consider f' simultaneously variable with a', equal and opposite to the variation of a', which is allowed. Therefore the condition $a' = b'$ makes undoubtedly the disturbances D_1' and D_3' minima. While the disturbance D_2', which contains R' in the denominator only, is not affected by this relation, but depends on the absolute value of b' only, which should be chosen as large as possible.

$a = b$ is therefore the *second* regularity condition, the fulfilment of which makes the relative disturbance of balance by a variation of K and λ as small as possible.

Substituting now $a' = b'$ in the expression of the D disturbances, and remembering that

$$R' = K',$$

we get
$$D_1' = s\,\lambda'\, v' \frac{J}{K'}\, \delta K'$$

$$D_2' = s\, \frac{J}{K'}\, \delta R'$$

$$D_3' = s\, v' \frac{T}{K'}\, \delta \lambda'$$

Thus D_1' and D_2', for constant $s\,\lambda'$ and v', become smallest the smaller $\frac{J}{K'}$ is, while D_3' becomes smallest the smaller $\frac{T}{K'}$ is.

Now remembering that

$$J = \frac{i + l'' + \rho''}{i}$$

$$K' = \frac{(l'' + \rho'')\,(i + l' + \rho') + i\,(l' + \rho')}{i + l'' + \rho''}$$

and $$T = L + \rho' + \rho'' + \frac{(l' + \rho')\,(l'' + \rho'')}{i}$$

$$\therefore \quad \frac{J}{K'} = \frac{(i + l'' + \rho'')^2}{i\,\{(l'' + \rho'')\,(i + l' + \rho') + i\,(l' + \rho')\}}$$

$$\frac{T}{K'} = \frac{i + l'' + \rho''}{i} = \frac{1}{\mu''}$$

For a tolerably good line $l'' + \rho''$ as well as $l' + \rho'$ can be taken as small in comparison with i; hence approximately

$$\frac{J}{K'} = \frac{1}{l' + l'' + \rho' + \rho''} = \frac{1}{L + \rho' + \rho''} \text{ and}$$

$$\frac{T}{K'} = 1$$

From which it follows that also for the compensation method ρ' and ρ'' should be selected as large as possible.

But $\rho = a + f$ does not give a condition, besides that we know we should select a and f absolutely *not* small.

Further we see that the disturbance D_3' has v' for its factor, while D_1' has $\lambda'\,v'$ for its factor.

Hence for a given $\lambda'\,v'$ the best will be to make v' as small as possible.

The regularity of the signals is therefore obtained if we fulfil the following conditions in either station:

$R = K$,
$a = b$,
ρ as large as possible,
v as small as possible.

Knowing this we may now consider that balance in either station is rigidly obtained, or that

$$R\sqrt{a} - K\,\lambda\,v\,\sqrt{b} = o$$

but $$R = K$$

and $$a = b$$

we have $$\lambda\,v = 1$$

The absolute value of a may now be determined by considering that it is advisable to produce the signals in either station in the most economical manner.

Maximum Magnetic Moment.

We have
$$P' = \frac{e''}{a'' + f'' + c''} \mu' q' \sqrt{a'}$$
$$P'' = \frac{e'}{a' + f' + c'} \mu'' q'' \sqrt{a''}$$

But
$$\frac{\mu'}{a'' + f'' + c''} = \frac{\mu''}{a' + f' + c} = \frac{i}{Q}$$

where
$$Q = i\,(L + \rho' + \rho'') + (l' + \rho')\,(l'' + \rho'')$$

$\therefore$
$$P = P' + P'' = i\,\frac{e'' q' \sqrt{a'} + e' q'' \sqrt{a''}}{Q}$$

which has a maximum for a' and a'' taken as independent variables.

If we, for instances, take $i = \infty$, than
$$P = \frac{e\,q\,\sqrt{a}}{L + 2\,(a + f)}$$

$\therefore$ $a = \frac{L}{2} + f$ for a perfect line, and by inference

$$\left.\begin{aligned} a' &= \frac{L'}{2} + f' \\ a'' &= \frac{L''}{2} + f'' \end{aligned}\right\} \text{approximately.}$$

Now we can decide on the method to be adopted for re-adjusting balance. On account of the regularity condition $R = K$, and as both undergo variation, especially K, we are obliged to adjust balance in the compensation branch by varying the resistance d, and leave the coils or their armatures stationary.

Thus the general solution of the first problem for the compensation method is:

1. Re-adjustment of balance is to be effected by a variation of resistance in the compensation circuit and *not* by a movement of the coils or their armatures. By this adjustment R is kept equal to K permanently, no matter in which branch the variation takes place.

2. $$\mathbf{f} = \mathbf{w} + \beta$$

$$\mathbf{a} = \mathbf{b} = \frac{\mathbf{L}}{2} + \mathbf{f}$$

$$\mathbf{v}\lambda = 1$$

v as small as possible and λ as large as possible.

β is known from the number and nature of the single cells of which the battery has to consist to produce through the given line (connected up in a circuit like fig. 3) single signals with sufficient strength.

w is known from the absolute largest variation β may undergo in time; hence f is determined and therefore also a and b.

Determination of λ and v.

We know that $\lambda\, v = 1$, and further that $\lambda = \frac{E}{e}$ should be selected as large as possible or v as small as possible, but otherwise it appears that no fixed values for λ and v can be ascertained. If we however consider the nature of the variations of R and K, which may disturb the balance, viz., those variations of R and K which are due to unavoidable decrease of the internal resistance of the two batteries by the working currents, it will be seen that a best value of λ does exist, and that therefore v also becomes fixed.

Suppose that at a certain moment

$R = K$ is rigidly fulfilled, and remembering that

$$R = b + d + \alpha$$

$$K = 2\,(a + f) + L \text{ (for a perfect line, } i.e., i = \infty)$$

and that further

$$a = b$$

and $$f = w + \beta$$

we have $\mathrm{d} + \alpha = a + 2\,w + 2\,\beta + L.$

Now, in this equation suppose everything constant except α and β, the internal resistance of the two batteries E and e respectively. Hence if we could achieve that

$$\delta\,\alpha = 2\,\delta\,\beta \text{ invariably,}$$

the variation of the internal resistance of the two batteries would not disturb the equation $R = K$, and therefore also not affect the

balance. With absolute certainty we cannot fulfil this desirable relation between the two variations, but with some probability we may. For it is well known that the internal resistance of a galvanic battery decreases in time by the current passing through the battery. Hence, if we suppose that the two batteries consist of identical cells (equal in nature, size, and internal resistance), we may say that the variation of the internal resistance of a single cell by the unit current in the unit of time is the same for both the batteries. Further if we make the other not improbable supposition, that the variation at any one time is proportional to the current which passes at that time, we have

$$\delta = \epsilon E . \frac{E}{R + \delta R} \phi^{(t)} = \epsilon \frac{E^2}{R} \phi^{(t)}$$

and

$$2 \delta \beta = \epsilon e . \frac{e}{K + \delta K} \phi^{(t)} = \epsilon \frac{e^2}{K} \phi^{(t)}$$

where ϵ is the variation of the internal resistance of a single cell in unit of time by unit of current; $\phi^{(t)}$ a certain unknown function of the time which, as the two batteries are working simultaneously, is not required to be known.

Hence from $\delta a = 2 \delta \beta$

and $K = R$

it follows that $\lambda = \frac{\mathbf{E}}{\mathbf{e}} = \sqrt{2}$

and $\mathbf{v} = \frac{\mathbf{r}}{\mathbf{q}} = \sqrt{\tfrac{1}{2}}.$

These values of λ and v bring the compensation method, with respect to regularity of working, as close to the differential method as is possible for us to do. For the disturbance of balance in the sending station by the steady decrease of the internal resistance of the two batteries has now been *probably* eliminated, which defect is excluded from the other two methods, by their own nature. There are then remaining only those variations of the battery resistance which do not follow the law of steady decrease, but which are more accidental, and make therefore the compensation method still inferior to either the differential or bridge method.

Physical meaning of $v = \sqrt{\frac{1}{2}}$

It has been proved that balance in each station is to be established by adjusting resistance and *not* by a movement of the coils or their armatures. Hence it will be practical and convenient to coil the two helices above each other, and have them acting on one and the same iron core.

Further as $v = \frac{r}{q} = \sqrt{\frac{1}{2}}$, it follows that the magnetic action of the a coil must be made greater than that of the b coil. Therefore it will be best to coil the helix b on the top of the helix a.

Further the magnetic action of a cylindrical coil of resistance a (in Siemens' units) can be expressed as follows:

$$m = s\sqrt{a}\sqrt{\frac{A\,\lambda}{c\,l}}$$

where A is half the cross section of the coil (cut by a plane through the axis of the coil) expressed in $\square^{mm.}$

λ the absolute conductivity of the wire material ($H_g = 1$ at $0°$ $C.$)

l the length of an average convolution expressed in metres.

s the magnetic force exerted by an average convolution of the coil when the unit of current passes.

c a coefficient representing the manner of coiling.

Hence for the a coil we have

$$m_a = s'\sqrt{a}\sqrt{\frac{A'\,\lambda'}{c'\,l'}} = q\sqrt{a}$$

for the b coil

$$m_b = s''\sqrt{b}\sqrt{\frac{A''\,\lambda''}{c''\,l''}} = r\sqrt{b}$$

Dividing m_b by m_a, and remembering that by condition $a = b$, and that $\lambda' = \lambda''$, $c' = c''$ by necessity, we have:

$$v = \frac{r}{q} = \frac{s''}{s'}\sqrt{\frac{A''\,l'}{A'\,l''}}$$

As we have supposed that the magnetic action of any one

cylindrical coil is proportional to the magnetic action* of an average convolution it is also consistent to put $s' = s''$, and we have at last

$$\frac{A'' \, l'}{A' \, l''} = \frac{1}{2}$$

If now the two bobbins of the coils a and b are taken of equal length, and if the thickness of the a coil be d', the thickness of the b coil d'', and the diameter of the iron core $2\,r$, we have,

$$\frac{A''}{A'} = \frac{d''}{d'}.$$

$$l' = (2\,r + d')\,\pi$$

$$l'' = \{\,2\,(r + d') + d''\}\,\pi$$

$$\therefore \qquad (4\,r + d')\,d'' = 2\,d'\,(r + d')$$

This equation fixes the relative dimensions of the two bobbins and their cores in order to have $v = \sqrt{\tfrac{1}{2}}$

Suppose for instance we make $d' = d''$ arbitrarily† we get $2\,r = d$, and from it can be easily calculated that the diameter of the wire of the b coil should be about 19 per cent. larger than that of the a coil. The absolute diameter of the wire depends of course on the absolute dimensions of the bobbins, and on the resistance of

* Lenz and Jacobi have experimentally proved that, within certain limits, the magnetic force exerted by a convolution on its centre (iron core) is almost independent of the diameter of the convolution. These limits are generally fulfilled in telegraph construction. Hence the magnetic action of a coil can be put proportional to the magnetic action of *one* convolution. Theoretically this can of course not be true, for the magnetic force exerted by a convolution necessarily extends on both sides of the plane in which the convolution is situated. Therefore the wider a convolution is the less of its total force exerted will be made use of for producing magnetism in the iron core, and consequently the force exerted by a convolution on its centre must decrease with the diameter of the convolution. It appears, however, that this decrease is exceedingly slow, and in the present investigation it is considered unnecessary to be taken into account.

† I have not been able to find anywhere a definite law which connects the diameter of a coil with the diameter of the core acted upon. In Siemens' relay, an instrument so well considered in all its details of construction, the diameter of the coil is about three times the diameter of the core. In the absence of anything else on the subject I thought myself justified in using this proportion. Hence the substitution of $d' = d''$, which gives $d = 2\,r$, or total diameter of the a coil equal to three times the diameter of the iron core.

the line for which the instrument is to be used. But this question, although of practical importance, has nothing to do with the theory of duplex telegraphy. This settles the solution of the first problem of the compensation method.

OTHER METHODS. There have been suggested from time to time many other methods for duplex working. On a closer examination it will, however, be found that as a general rule they do not differ essentially from the three fundamental methods treated of. I shall therefore dispense with the labour of investigating these derived methods.

In case it should be thought necessary to investigate them, no difficulties ought to be met with, if only the general plan of attacking duplex problems be remembered, viz. draw the diagram of the method in its most general form; develope the forces p, P, and Q; from these three forces determine the functions S and D; find the relations which must hold between the different variables (resistances and E. M. F. S.) of which the system consists, in order to make S and D simultaneous minima; consider the question of *immediate balance* which determines also the best mode of adjusting balance; consider that the movement of the key must not alter the complex resistance of the station to which the key belongs, *i. e.*, that the working of the key must not affect the balance of the distant station; determine the absolute values of the different variables when balance is rigidly fulfilled by considering the question of economy, *i. e.*, establish the relations for maximum currents and maximum magnetic moments; any variables which should then be left indeterminate must be fixed by secondary consideration, and by certain practical conditions.

Before comparing quantitively the efficiency of the three fundamental methods treated of, it is required to solve two questions, viz. the E. M. F. required for each duplex method; the absolute size of the increments of the adjustable resistance.

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The Sixty-fifth Ordinary General Meeting was held on Wednesday, the 13th of March, 1878, Major Bateman-Champain, R.E., Vice-President, in the Chair.

The Secretary read the following paper:—

INSULATORS FOR AERIAL TELEGRAPH LINES.

By John Gavey, Member.

In venturing to bring before the Society the question of insulators for open-air lines, the author rather desires to draw general attention to the whole subject than hopes to be able to impart any new facts. That there is wide room for improvement in our present standard of insulation is, it is believed, generally admitted, and, inasmuch as it is frequently simply necessary to let it be clearly understood that a want is felt, in order to stir up inventive minds to satisfy that want, it is thought that some of the facts set forth below, and the valuable discussion that generally follows a paper on a practical subject of this character, may result in drawing attention more fully to the whole question.

In dealing with the subject, it will perhaps be advisable to consider, first, the principles which should guide us in designing or selecting an insulator; and these may conveniently be dealt with under two heads, viz.: The material of which the insulator is to be made, and the form to be given to it.

First, the material.—Its electrical resistance should be as nearly as possible infinite, for, obviously, unless it possesses this quality it will be useless as an insulator. It should be homogenous throughout its substance. It should not be porous, for if it be so, it will inevitably absorb moisture; and whatever be the specific resistance of the material itself, it will become a more or less good conductor when saturated with water; and the difficulty of wholly expelling moisture, when once absorbed, is perhaps not always fully appreciated. An illustration of this is given further on. It should be susceptible of having a high polish, or a smoothly glazed surface, in order to retard accumulations of dirt and dust, and to admit of all such accumulations being readily removed, partly by the natural action of rain-showers, and thoroughly when needed by hand-cleaning. It should not be subject to deterioration, either on its surface or internally, through atmospheric or electrical causes, through variations of temperature, through frost, through the action of acids and salts held in suspension, or through other like causes. It should be readily moulded into any form which it is desired to adopt, and it should retain that form unchangingly. It should have as slight an affinity for moisture as possible, so as to diminish the formation of conducting films over its surface when the atmosphere is saturated. Its tensile and compressive strength should be sufficient to admit of its withstanding the maximum strains to which it is likely to be exposed in practice, without deterioration of any kind, and lastly its toughness should enable it to withstand with impunity ordinary blows and the rough usage to which engineering materials are always more or less exposed.

The evil of porosity in any material employed for insulating purposes is doubtless well appreciated, but it may not be so generally understood how difficult it is to eliminate moisture once it has penetrated the pores of an insulating substance. The following experiments bear on this subject. Following some published results by Du Moncel, on the polarization and conductivity of minerals, certain experiments were made by the author for Mr. W. H. Preece, with the object of ascertaining how far the conductivity arose from absorbed moisture and how far from the materials themselves. The following specimens were selected, cut

evenly and ground down smoothly on two opposite faces. They were then carefully washed to free them from grease, oxide, and other possible disturbing elements, laid on a clean glass plate, and left untouched, in an ordinary room without fire, for three days, to allow them to dry.

The stones experimented on consisted of the following :—

1. Bath oolite.
2. Alabaster.
3. Reddish lias.
4. Red sandstone.
5. Reddish lias.
6. Bridgend lias.
7. Common sandstone.
8. Pennant sandstone.

On testing them with a Thomson's galvanometer, having a constant of 148° with one cell through 10 megohms, at the end of the three days all the specimens gave full deflections, with 289 cells and 1,000 shunt, platinum electrodes being attached on either side of each stone. They were all then exposed to heat, by suspending the glass plate which held them two inches above an ordinary closed iron stove, which was kept intensely heated for a period of six hours. The temperature of each stone, having then for a considerable time been such that it would not bear touching with the hand, readings were again taken, with the same constant and battery power, the following being the results :—

No.	First deflection.	Deflection after 1 minute's duration of Current.
1	Nil.	Nil.
2	Nil.	Nil.
3	320°	900°
4	170,000°	29,000°
5	450°	650°
6	80°	85°
7	200°	150°
8	50°	120°

A further exposure in the same manner, to similar heat, on the following day for eight hours, resulted, at the end of that time, in the following readings:—

No.	First deflection.	Deflection after 1 minute's duration of Current.
1	Nil.	Nil.
2	Nil.	Nil.
3	25°	40°
4	110°	230°
5	8°	3°
6	4°	4°
7	Nil.	Nil.
8	10°	10°

The specimens were then laid on a sheet of clean iron, directly on the top of the stove, so as to increase the temperature still further, when all traces of conductivity finally disappeared, after a lengthened exposure to a temperature which sufficed to decompose the alabaster. Other specimens of different stones subsequently experimented with acted in a similar manner. It may therefore safely be assumed that no ordinary natural temperature, in this country, would suffice to wholly expel absorbed moisture from a porous substance.

In practice, but few materials have been employed in the manufacture of insulators. They may be said to have been limited to glass, porcelain, earthenware, ebonite, and wood saturated with insulating compounds.

Glass, originally one of the first materials used, was rejected in this country, on account of its readily condensing films of moisture over its surface, and also because of its brittleness and its tendency to fracture under variations of temperature. It is now, however, used in America and Switzerland to a considerable extent, and some of the results obtained with it are said to be good. It possesses, in a high degree, many of the requisite qualities of a material for an insulator. Its electrical resistance to direct conduction is almost infinite. It is non-porous, homogeneous, highly polished on its surface, readily moulded into any given shape, and easily manu-

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factured of a given quality, but unfortunately those objections already mentioned were found so strong as to lead to its entire rejection in England.

Porcelain has perhaps been more widely adopted than any other material, and, if properly selected, well manufactured, thoroughly burnt, so that, a partial vitrification having taken place throughout, it becomes homogeneous and non-porous, it probably affords the best material hitherto used. If of really good quality, it satisfies many of the conditions laid down, its principal defect being its brittleness. Of course much depends on the selection of the ingredients of which it is made and on the care devoted to its manufacture, but even when this is observed to the utmost, the insulators made of it should be subjected to searching tests before being accepted.

According to the description of clay used, and the different proportions in which this is mixed with the powdered flint and other materials employed, will the porcelain be hard or soft. The proportions used are generally trade secrets, each maker having his special formula for mixing. That manufactured in Prussia, the Berlin porcelain, is of a very fine description, extremely hard, and is much liked by some.

In this country a softer porcelain generally is used for insulators. It is probable that the harder German porcelain gives a higher specific resistance than English porcelain generally, but the latter can be selected so as to give such a high result as to be practically infinite, in comparison with the absolute resistance of an ordinary insulator, which is comparatively low owing to surface leakage. There is no evidence to show that the softer qualities deteriorate more rapidly, under atmospheric or electrical influences, than the harder ones.

The final test for porcelain, as for any other material used for insulators, is of course the electrical one; for invisible fissures, porosity, or other imperfections, are thereby detected; but a careful examination of the fractured sections of an insulator will generally give some idea of its quality. If properly made up and sufficiently fired the fractured surfaces exposed will be more or less conchoidal, smooth, even, and homogeneous in appearance. Any departure from this evidently indicates imperfection.

I am indebted to Mr. Bell, the superintendent of the factories in the Telegraph Department of the Post Office, who has necessarily had a wide experience in this direction, for the following brief summary of the causes of the low resistance of the insulators which are rejected after being exposed to the ordinary tests.

First. Flints not ground fine enough to make with the other constituents of the porcelain a smooth paste. The fractured section of the ware, when examined with a lens, is roughly comparable to a quarry of chalk with its intermixed flints.

Second. Insufficient firing to fuse the flux.

Third. Excessive firing, which makes the ware spongy.

It will be seen that these are merely defects in manufacture, preventible by the adoption of proper precaution; and as they are readily detected they do not militate against the use of the material itself.

Stoneware, which has been employed to a considerable extent in this country, is perhaps of a more variable character than porcelain, but if carefully selected, moulded under a considerable pressure, and well burnt, it falls very little short of porcelain as a material for insulators; and its unpretending colour appears in many localities to insure its freedom from the stone-throwing proclivities of the idle and mischievous, whilst the staring white of the porcelain appears irresistible. In respect of theoretical conditions, well selected and manufactured stoneware stands about on a par with porcelain. Many of the remarks as to the manufacture of porcelain will apply to that of stoneware.

Ebonite, which appeared a most promising material, and was at one period somewhat extensively used, failed through its rapid surface deterioration under the influence of the atmosphere. This is fatal to its employment for out-door insulation. Could this be overcome it would be a most valuable substance on account of its high insulating qualities and its small affinity for moisture, and its use would then probably admit of considerable improvements in the present standard of out-door insulation.

Wood, saturated with insulating substances, has been tried. It is questionable whether a high-class insulator could with safety be turned out of this material. It is extremely difficult to deprive a very

porous and hygrometrical substance like wood of its strong tendency for absorbing atmospheric moisture, in fact it is a question whether it would be possible to entirely fill up its pores, and the freedom from malicious fracture, which its use would carry with it, would be dearly purchased at the cost of the lowered insulation which would certainly follow any failure in the attempt to render it impermeable.

In the second place we have to deal with the form of the insulator. Taking the resistance through the substance of the material selected as approximately infinite, which it would be practically in any but a defective specimen, the conductivity of an insulator arises through the deposition of a film of moisture over its surface. Now the ordinary law, that the resistance of a conductor varies directly as its length and inversely as its section will evidently apply in this case as in others, and therefore in calculating the resistance of a given insulator, or, in other words, the resistance of the film of moisture condensed on it, we have for its length the distance over its surface from the point where the wire is attached, to the point where the insulator is affixed to the pole; and for its section the thickness of the film of moisture multiplied by the mean circumference of the insulator. Assuming, therefore, that under like atmospheric conditions the thickness of the film of moisture deposited on various forms of insulators of the same material will be constant, the resistance of each insulator will vary directly as its length and inversely as its mean circumference. This law in practice applies with accuracy only to simple cylinders, for in complex forms of insulators other disturbing causes are introduced, which will appear further on.

The theoretical conditions to be aimed at, therefore, in designing the form of an insulator, may be enumerated as follows:—

The maximum resistance should be obtained by any of the various means which may be suggested, viz., by increasing the length to be traversed by the current, whilst diminishing the section of the conducting film; by the retention of a dry surface on one portion of the insulator, either during heavy rains, mist, or fog, or if possible under all circumstances; by the adoption of a form that will not aid or retain deposits of dust, soot, or other materials, which if not conductors *per se* act very injuriously, by retaining and in-

creasing the thickness of the moisture films; and also one that will not foster accumulations of the cocoons of spiders and other spinning insects. The insulator should admit of its surface being readily washed, naturally by heavy downpours of rain, and of thorough cleansing in all its parts, when necessary, by the lineman who has charge of the length. The construction should be such that the partial or entire fracture of an insulator should not necessarily interrupt or materially impede the working of the line. It should admit of the conductor being readily and firmly attached to it, and should be so constructed that if a wire be broken in one span the insulators on each side of the break should retain the fractured wires, and not allow them to run back; and, finally, it should be readily attached to and removed from the supports or poles, without the aid of highly-skilled labour.

In the early days of telegraphy wires were insulated in this country with short cylinders of earthenware, pierced through the axis, and supported by a ring, which fitted a groove in the circumference. Various improvements, which scarcely require further attention here, were made in succeeding years, by which the standard of insulation was gradually raised, until in 1856 Mr. Latimer Clark introduced on the lines of the Electric and International Telegraph Company the well-known porcelain invert, which may be called the parent of a whole generation of the modern form at present in use. This invert is so well known as to need but the most brief description. It consisted of a so-called double-cup insulator, of the section showed in fig. 22 (Plate 4), supported on a vertical pin, and containing a deep groove in the top to carry the conductor. These were largely used in England at the time they were first made, but they were subsequently replaced by Varley's well-known earthenware insulator (fig. 2), in which the two cups were altered in shape, prepared in separate pieces, and cemented together, so that a flaw or fracture in one would not destroy the insulating power of the whole.

In this country until recently Varley's double-cup earthenware insulator (fig. 2) and the double cup porcelain insulator shown in fig. 3 have been exclusively employed for all main circuits. For unimportant branch circuits single-cup insulators (figs. 4 and 9)

have been employed, which afford sufficient insulation for the short lines on which they are used. These single-cup insulators are made both in porcelain and earthenware. All the insulators hitherto employed in Great Britain are supported by means of galvanized iron bolts, cemented at one end into the interior cup of the insulator, and provided at the other end with a thread and nut for fastening to the arms or brackets used for attachment to the poles.

The postal telegraph department of Great Britain, recognising the importance of obtaining the highest insulation possible for its lines, has lately introduced a new insulator of the double-cup form (fig. 6), in which the length is considerably increased, and which is attached to the bolt by means of a female screw in the interior of the cup, fitting a corresponding thread at the upper extremity of the bolt. An india-rubber washer, placed between the lower end of the insulator thread, and a flange on the bolt, prevents fracture of the insulator by over-screwing. This method of attachment will admit of the ready removal of the insulator for thorough cleansing and testing at intervals. This insulator is intended for use on long important circuits. A modification of the old form (fig. 3), likewise provided with a screw bolt, is intended for use on lines of moderate length.

In Prussia the porcelain invert already referred to was adopted early after its invention, and the present form in use in that country, which is illustrated in fig. 20, may be said to be but a modification of the original, the weak parts being strengthened so as to better resist the effects of ill usage, and the sections lengthened to increase insulation. These insulators, or modifications of them, have been introduced in many continental countries, amongst others, throughout the German Confederation, Russia, Sweden, Denmark, Italy, and Spain. They are likewise used by the Indian Government Telegraph Department.

In France, a few years ago, an insulator of a very inferior character was employed. Since then, however, the French have assimilated their systems to those of other European countries, having adopted a form of double-cup insulator for all important circuits, and one with a single cup for less important lines, viz. for lines varying from 50 to 200 kilomètres in length.

In Spain, Siemens' insulators were, until recently, almost wholly employed, but now the Prussian pattern, simply modified, so that a groove in the top of the insulator supports the wire, is used. The groove is necessary, because in that country the old system of winders is still in existence. Insulators of the Prussian form, fitted on the upper extremities with two winding drums, are placed at intervals of one kilomètre apart, the line-wire running loosely through the grooves in the intermediate insulators.

In America, glass, as has already been mentioned, is almost wholly employed. The form mostly adopted consists of a single glass cup, in shape somewhat similar to the outer cup of a Varley's insulator. The interior of the cup is fitted with a female screw, into which is fixed a wooden pin, which supports the insulator. The disadvantages of glass have already been mentioned, and they are to some extent admitted by American electricians, though it is said that their insulators are not so liable to fracture as is generally supposed. Prescott gives the average number replaced for four years on the Western Union Telegraph Company's system at 6·4 per cent. per annum. This is higher than it is in this country on our road lines, although the American lines referred to mostly follow railway routes, where they are not so liable to malicious injury and wilful fracture of insulators as on roads through thickly populated districts.

An insulator consisting of white wood, saturated with an insulating compound, the top protected with a metal cup, called the Kenosha, has likewise been somewhat largely used.

Brook's insulator consists of a cylindrical iron case, in which is inserted a blown glass bottle of peculiar form. Inside this is fastened a pin, which, terminating in a species of double hook, forms a support for the wire. It is claimed that a surface of blown glass, which is cooled by air contact alone, offers particular advantages in resisting deposits of moisture and dirt. This insulator has given some very good results, but it has not been practically tried in Europe against the forms in general use in the old world.

It has already been pointed out how important a matter in connection with line insulators is that of testing; and in considering this question it naturally divides itself into two heads, viz., the

testing of insulators prior to issue, so as to insure the employment of effective ones; and, secondly, the tests made periodically to maintain the efficiency of the line.

A description of the method of testing insulators, prior to their use on the Indian Government lines, was given by Mr. Ayrton in a paper read before the Society in the year 1873. In the authorised instructions for testing, published by Mr. L. Schwendler in 1876, the minimum resistance which is accepted in an insulator by that department is set down at 2,000 megohms, with one minute's electrification, after the insulator has been immersed in water twenty-four hours.

The following is the method of testing at the chief factory of the Post Office in London.

All insulators are deposited in tanks, and filled with water, both inside and out, to within three-quarters of an inch of the lips. To prevent surface leakage, which would always exist in damp weather, they are kept dry on the edges by the heat of numerous jets of lighted gas, placed just above them, or the same result may be obtained by keeping the atmosphere of the testing-room artificially dry by means of hot water pipes. After soaking for twelve hours, they are tested by means of a Thomson's galvanometer and 140 Daniell's cells. The full constant of the instrument is generally 70,000 megohms for 1° deflection. To protect the galvanometer coils from powerful accidental currents, and to insure speed in testing the insulators, an ingenious combination of three keys and shunts is employed. In the normal position the galvanometer is short circuited. On depressing No. 1 key $\frac{9999}{10000}$ of the current is shunted from the instrument. No. 2 key introduces a shunt of $\frac{99}{100}$, and No. 3 gives the full current. The advantages of such an arrangement will be patent to all who have had to execute rapid tests with a high battery power and a delicate galvanometer. If a deflection be given with No. 1 key, the insulator is at once rejected, unless the lips are observed to be wet. If the latter be the case, or if deflections be given on depressing keys No. 2 or 3, the insulator is marked, and subsequently dried and carefully re-tested. Should any leakage still be shown, it is then finally rejected. By this means all defects of manufacture or accidental flaws, are

inevitably detected before the materials are passed into actual employment.

In France a somewhat similar system of testing insulators prior to use is adopted, the standard being 7,000 megohms.

However important it may be to insure the use of none but the best material in the original construction of the line, it is equally important that proper steps be taken to maintain its insulation at a certain standard, beyond which it should never be allowed to drop. The causes of the lowering of insulation in a working line will be glanced at later, but the regular and frequent testing of all working circuits affords the only data by which the officer in charge can keep himself *au courant* with the gradual deterioration which sets in from the first; and which will enable him to take whatever steps are needed, when action becomes necessary, to remove defects before they cause actual interruptions.

Having glanced somewhat briefly at the theoretical conditions to be aimed at, and next at the practical applications of these conditions as they have been carried out in various countries, it remains to consider how far we have succeeded in reaching a point of perfection, with which we may be satisfied.

Now, all who have to deal with practical telegraphy are well aware that sudden and extreme variations in the insulation of working circuits are of frequent occurrence, but it is not often that opportunities arise for taking careful measurements, on an extended scale, of these variations, as all available wires are generally fully employed in the transmission of messages. The introduction of duplex working has familiarised all those who have to deal with wires worked on that system with the nature of these changes, but during the erection of a main line from London to Bristol, a few years ago, an opportunity arose for carrying out a series of tests and experiments on varying lengths of completed wires, quite free from the disturbing causes introduced by the proximity of working circuits; and Mr. W. H. Preece accordingly caused an extended series of a variety of kinds to be made. Amongst others, insulation measurements at fixed periods, with the corresponding wet and dry bulb readings, were taken, and to illustrate the connection between the two, and to exhibit graphically the

changes that take place, the curves representing the periodical observations have now been plotted out, and are shown in the annexed diagrams.

The vertical sectional spaces represent days, the horizontal ones degrees of moisture, and of insulation per mile. Taking a saturated atmosphere at 100°, each horizontal space represents twelve degrees of moisture, and likewise 2·4 megohms for insulation. Of course the two series vary inversely as one another, but in order to follow the connection more readily, the moisture curve is inverted, so that they both appear in one direction.

The first curve represents tests taken at Marshfield, midway between Bristol and Chippenham. Six wires, each nine miles long, extending from Marshfield towards Bristol, were looped into a continuous circuit 54 miles in length, and the insulation curve is represented by a dotted line. Between Marshfield and Chippenham six wires, each 12 miles long, were looped into a 72-mile circuit; and the result is shown by the dash and dot line on the diagrams. A wet and dry bulb thermometer at Marshfield recorded the moisture, and this appears as a full black curve in the upper portion of the engraving.

In the second and third diagrams the insulation tests of two wires, looped from Marlborough to Calne, the loop being 26 miles long, are recorded. The curve is shewn by a dotted line. Wet and dry bulb readings, reduced to degrees of saturation, were taken at Marlborough in the second, and at Marlborough and Calne in the third case; and these curves appear as full black and dash and dot lines respectively. Observations were mostly taken by day, at intervals of two hours, but night observations are likewise recorded. The lines were insulated with Varley's double cup-earthenware insulators, a few shackles being interposed at heavy angles.

It may here be convenient to roughly tabulate the observed variations in insulation, for more ready consideration, leaving the actual figures recorded to be obtained from the curves.

Marshfield Tests.

Near Chippenham to near Bristol.

July 3rd. High at 1 p.m.; rising till 3 p.m.; falling at 8 p.m.

" 4th. High at 7 a.m.; rising till 9 a.m.; steady till 10 a.m.; varying till 1 p.m.; rising high till 3 p.m.; falling steadily till 10 p.m.

" 5th. High at 8 a.m.; the mean of the two steady till noon; rising till 5 p.m.; then steady fall till midnight.

" 6th. High at 9 a.m.; falling till 11 a.m.; very high at noon; abrupt fall at 1 p.m.; falling till 5 p.m.; rising till 7 p.m.; then falling again.

" 7th. Variable.

Marlborough Tests.

No. 2 diagram.

Sept. 30th. High at noon; down at 3 p.m.; varying afterwards.

Oct. 1st. Falling till 11 a.m.; fairly high at 1 p.m.; then falling.

" 2nd & 3rd. Low at 9 a.m.; high at noon; low at 8 p.m.; falling; still 1 a.m., when very low; commenced

" 3rd. rising 3 a.m.; rising slowly till 7 a.m.; no further observation on the 3rd.

" 4th. Very high 9 a.m. till 3 p.m.; then sudden drop at 5 p.m.; still falling at 7 p.m.

" 5th. Fairly high at 9 a,m.; varying all day.

" 6th. Sunday, no observations.

" 7th. High 9 a.m.; rising till 11 a.m.; steady till 1 p.m.; abrupt fall at 5 p.m.; still falling at 7 p.m.

" 8th. Low at 9 a.m.; rising at 11 a.m.; very high at 1 p.m.; steady till 3 p.m.; abrupt fall at 5 p.m.; still falling at 7 p.m.

" 9th. Very low 9 a.m. till 11 a.m.; at 1 p.m. slight rise; continued at 3 p.m.; very high at 5 p.m.; dropping again at 7 p.m.

" 10th. Very high 9 a.m. to 11 a.m.; very abrupt drop at 1 p.m.; still falling at 3 p.m.; rising at 5 p.m.; down again at 7 p.m.

" 11th. High at 9 a.m.; rising at 11 a.m.; very high 1 to 3 p.m.; abrupt fall at 5 p.m.; still falling at 7 p.m.

Marlborough Tests.

No. 3 diagram.

Nov. 1st. High at 11 a.m.; falling at 3 p.m.; still falling at 5 p.m.

" 2nd. Low at 9 a.m.; rising at 11 a.m.; very high 1 p.m. to 3 p.m.; sudden fall at 5 p.m.; slight rise at 7 p.m.

" 3rd. Sunday; no observation.

" 4th. Very high at 9 a.m.; considerable fall at 11 a.m.; slight rise at 1 p.m.; sudden fall at 3 p.m.; continued fall till 8 p.m.

" 5th. A showery day, causing continued variations.

" 6th. Variable.

An examination of these results shows graphically what might have been expected from theoretical considerations. Briefly it appears:—

First, that there is as it were a great wave of moisture that sweeps daily over the land, having its maximum near midnight, and its minimum at noon; and that accordingly the insulation of our circuits, generally low from 7 to 9 a.m., rises to a maximum from 11 a.m. to 3 p.m., then abruptly falls, reaching a minimum between 7 p.m. and midnight.

Secondly, that very frequently the most abrupt changes in insulation take place in a most limited time; the resistance dropping from several megohms to a fraction of a megohm per mile in the course of an hour or two.

Thirdly, that the resemblance between the insulation and moisture curves is very remarkable; so much so, in fact, that it is not improbable that an extended series of insulation readings would form a more accurate register of moisture over any given extent of country than ordinary hygrometrical readings.

The almost simultaneous movements in both curves is not the least noticeable feature in the case; divergencies, when they occur, being readily accounted for by comparing the extent of line exposed

to a varying atmosphere, the moisture in which was only recorded by one, or at the most two, hygrometers.

Perhaps the most useful lesson taught by these diagrams consists in the striking indication of the rapid variations which frequently takes place in the insulation of some of our best lines, in the course of a very limited period. This rapid variation in insulation, and consequently in the strength of the currents received at either end of a line, is perhaps one of the greatest obstacles to the introduction of what may be termed more refined modes of telegraphy than are at present practised. Given a certain standard of insulation that is more or less invariable, and provision can be made either in the shape of increased battery power, or increased delicacy in the receiving apparatus, to meet a given constant loss of current, but let the insulation vary within wide limits in any given period of time, and the choice of apparatus and systems of working are limited to a very considerable degree. With ordinary systems of working, very wide variations in insulation may take place without being attended with any evil results, a simple turn of a screw serving to adjust so as to meet the change; but the gradual tendency of the day is the introduction of new modes of working which demand a greater steadiness and invariability in the strength of the currents circulating through the line, and hence the desirability of concentrating attention on the whole subject of insulation.

As further illustrating the variations in the insulation resistance of lines to which in this country we are subject, the following table has been compiled from the Board of Trade meteorological observations at Portishead for the year ended October 31st, 1877. It shows the degrees of moisture in the atmosphere during the morning and evening observations, and the number of days in each month when rain fell within the twenty-four hours. From this it will be seen that rain fell on 202 days out of the 365; that there were 163 days when in the morning, and 106 when in the evening the amount of moisture in suspension was over 90 per cent of saturation, a degree of humidity which, as our curves show, causes a lamentable fall in insulation. Only 51 days in the morning and 110 in the evening give less than 80 per cent. of moisture, a degree when fair insulation may be anticipated.

Degrees of Humidity of the Atmosphere for Twelve Months ending October 1877.

Meteorological Observations at Portishead.

Month.	Number of days in each month and degrees of humidity.								
	Morning readings.				Evening readings.				Rainy days.
	Saturated.	90° to 100°	80° to 90°	Below 80°	Saturated.	90° to 100°	80° to 90°	Below 80°	
	days.	days.	days.	days.	days.	days.	days.	days.	
1876 Nov.	13	8	8	1	3	11	12	4	17
Dec.	7	15	9	0	4	11	15	1	23
1877 Jan.	11	12	7	1	7	14	8	2	25
Feb.	8	4	10	6	3	11	9	5	20
March	6	7	14	4	3	4	16	8	18
April	7	8	13	2	1	2	9	18	20
May	0	4	13	14	0	2	11	18	11
June	2	5	11	12	0	2	9	19	12
July	2	6	16	7	3	4	9	15	17
August	0	5	24	2	2	2	15	12	17
Sept.	7	8	15	0	1	7	16	6	8
Oct.	9	9	11	2	2	7	20	2	14
	72	91	151	51	29	77	149	110	202

A careful consideration of this table, in conjection with the curves of moisture and insulation, will give a graphic idea of the variability of our working circuits, especially if allowance be made for the weak points in a line, such as the growth of trees, fractured insulators, and other defects which even with the best maintenance occasionally creep in, and cause these variations to reach still wider limits.

The insulation curves justify in a remarkable degree the hour selected for the taking of morning tests in this country, as they demonstrate strongly the fact that they take the lines in their worst state, and therefore give prominence to all defects, incipient or pronounced.

Now no doubt the evils referred to above have been more or less widely felt, and attempts have from time to time been made to improve the standard of insulation, but, with few exceptions, all these attempts have been made in one groove. The original double cup has been taken as the *one* pattern, and it has been modified in a variety of forms. No doubt some of these present advantages over the others, either by increasing the length, or by diminishing the mean circumference, or by resisting more readily accumulations of dirt, or by providing for the cleansing action of heavy rain, but the whole of these must, from their construction, be liable to those constant resistance variations, to a greater or less extent, which have just been dwelt on; and the most that has been, or is likely to be, accomplished in this direction is the raising to some extent of the minimum insulations given in the worst weather. After extended exposure they all, however, show a lamentable falling off from the high resistance they offer when sent out from the factory.

In 1876 the author, under the instructions of Mr. W. H. Preece, commenced a series of tests at Bristol on a number of different forms of insulators, the whole of which are shown on the appended table. The insulators were attached to arms in the usual manner, and fixed on a lofty pole in the store-yard of the Bristol depôt. Each lot of insulators of the same description was ranged in vertical order, so as to expose all to the same influences, connected together by a line-wire, and the bolts placed in contact with an earth-wire. When tested, a well-insulated leading wire was connected to

the line-wire of each set successively, and the reading recorded by a Thomson's galvanometer. These tests were continued at periods up to the end of the year 1877. Unfortunately all the insulators were not set up together, so that it would not afford a just comparison if the whole of the tests were here tabulated; but the annexed table shows the minimum resistance given by 10 insulators of each class during the tests taken in the periods mentioned opposite each.

MINIMUM RESISTANCE OF 10 INSULATORS OF EACH CLASS TESTED.

Description of Insulator.	No. of Figure.	Minimum Resistance of 10 Insulators.	Duration of Tests.
Porcelain D.S. Terminal Insulator No. 1	1	·124	May 30, 1876, to Oct. 29, 1877
Earthenware D.S. Varley's	2	·335	do.
Porcelain D.S. P.O. form	3	·131	do.
Earthenware S.S. P.O. form	4	·073	do.
Porcelain D.S. Andrew's form	5	·182	do.
Porcelain Terminal Shackle	—	·014	do.
Porcelain D.S. Schomburg's ware... ...	7	·3	do.
Porcelain S.S. Schomburg's ware... ...	8	·128	do.
Porcelain S.S. P.O. form	9	·076	do.
Earthenware Umbrella (old No. 3) ...	—	·040	do.
Porcelain D.S. Indian Govt. form ...	11	·150	Feb. 3 to Oct. 29, 1877
Porcelain S.S. Andrew's (new)	12	·250	do.
Porcelain D.S. Corrugated Exterior ...	15	·187	July 1, 1876, to Oct. 29, 1877
Porcelain D.S. Fuller's Inverted Cone ...	14	·268	do.
Porcelain D.S. Schomburg's ware ...	13	·281	Feb. 3 to Oct. 29, 1877
Porcelain D.S. P.O. form	3	·223	Jan. 1 to Oct. 29, 1877
Porcelain D.S. iron-capped (perforated)...	16	·225	do.
Porcelain D.S. Fuller's and Langdon's ...	19	·985	Dec. 10, 1876, to Oct. 29, 1877
Porcelain D.S. Prussian form	20	·130	Feb. 3, 1877, to Oct. 29, 1877

It would however be manifestly unjust to take the figures opposite the insulators in the foregoing table as accurately recording the exact insulation which a line fitted with each class would reach in the course of a greater or lesser period, or to assume that these figures represent the actual relative value of each insulator. Comparative tests of various classes of insulators, on a limited scale like the present ones, are of considerable service, if accepted with due caution; but an unhesitating reliance in isolated tests of this character might readily involve serious errors. It must always be borne in mind that a fault developed in one insulator, which may be quite independent of its form, material, or general value, might at once cause the recorded results to drop materially; or in such tests as those referred to above, some accidental cause, such as a gust of wind carrying water with it from an adjacent building might saturate one set whilst leaving others comparatively unaffected. On the other hand, an accidental circumstance, such as a film of grease, even from the hand of the man setting up the insulators, and invisible on superficial inspection, might raise an insulator, for a considerable time, above its real value. Of course the tests of these insulators may be considered severe ones, for in the neighbourhood of a town like Bristol all the protected portions of an insulator, such as the inner cups, rapidly become coated with deposits of carbon, of dust, and of all the other particles that float about in the atmosphere of a manufacturing town. Even the outer portions of the insulators lose their brilliancy, and become soiled very speedily, so that the recorded insulation would naturally be lower than that given by lines running through the open country.

In the open country, however, it is only a question of time for the best insulator of the ordinary pattern to deteriorate to a very considerable extent. When newly set up, in ordinary rain the outer cup only is thoroughly wet, and the moisture on the inner cup is simply that deposited, in an extremely thin coating, from the suspended vapour in the atmosphere. As time goes by, however, deposits of all kinds accumulate, some more or less conducting, others simply acting injuriously, by increasing the thickness of moisture films. This process continues until it becomes neces-

sary to clean the insulators throughout, when, for a short period, a high standard is again reached, but immediately deterioration again sets in, and the same process is repeated.

Now the results given in the above table show most plainly that even amongst those insulators that are supposed to take the highest rank a period arrives when the insulation occasionally becomes so low through general leakage that the working of very long lines at a high rate of speed would be seriously interfered with if not interrupted; and, inasmuch as there is a limit, which perhaps has in many instances been reached, beyond which an increase in length, size, or weight of an insulator would introduce evils which would counterbalance the advantages to be obtained from its increased resistance, it is evident that if any very marked improvement is sought for it must be in a new direction, and not in the same groove that has hitherto been so generally followed.

Perhaps the best results with the present system would be obtained by the adoption of a simple cylinder, with a very small diameter, and of the ordinary length. The idea appears to be prominent in the designing of Andrew's new form of insulator; but it is very doubtful if the system can be fairly tried with porcelain, as this substance necessitates making the diameter of the cylinder of considerable dimensions. Were it possible to obtain a material which admitted of the construction of an insulating cylinder, say one-fourth or one-third of an inch in diameter, and six or eight inches long, the wire being suspended at its extremity, probably higher results would be obtained than with even the best form of the ordinary class of insulators. Tensile strength could be given by an interior iron or steel wire, and provision could readily be made for obviating the effects of transverse strains. But for surface deterioration, ebonite would have been useful for such a purpose, and if such an idea can be ultimately applied it will probably be through the medium of one of the hydro-carbon insulators.

A new line of departure has been taken by Messrs. Johnson and Phillips, who, fully recognising the difficulty of achieving further improvements on the old lines, have boldly struck out into a new field. They have adopted as a principle the necessity for maintaining,

under all circumstances, one portion of an insulator, in an unchangeable and invariable state; and as the resistance of the insulator would depend almost wholly on the resistance of this invariable section, they would thereby obtain a practically constant insulation resistance on a working wire under all circumstances. Their method of obtaining this result consists in providing a well in the interior of each insulator, and filling this well with an oil, which having no affinity for moisture resists deposits from the atmosphere. It is so situated that direct access of water from rain-showers becomes very difficult if not impossible.

Some of the forms adopted for arriving at this result are illustrated in figs. 10, 21, and 23, (Plate 4).

Fig. 23 shows how the inventors propose applying their system to an ordinary insulator, by inverting it, and protecting the interior by means of a zinc covering.

Fig. 21 illustrates their No. 5 insulator. The lower edge, whilst in a plastic state, is turned upwards and inwards; so as to form the internal cell to be filled with the insulating oil; and another form is illustrated in fig. 10, in which an outer iron cup protects the whole from malicious injury; whilst the inner oil well is moveable up and down the bolt for filling, for inspection, &c., being held in its place by an india-rubber ring.

Mr. S. E. Phillips has supplied me with the two latter forms for experimental testing, and the results obtained are shown below.

In order to afford the means of a better comparison between the various forms of insulators than was possible with the original tests, the majority of those tested in 1876-77, referred to previously, were taken down, carefully washed, and refixed; a few not considered worth retesting being omitted. To these were added a new form of terminal insulator (fig. 18), introduced in the Postal service; the new insulator with screw bolts for long important circuits, and the oil-well insulators of Messrs. Johnson and Phillips.

In all 21 forms of insulators were submitted to tests under the same conditions as those previously enumerated.

They are all illustrated in section, in figs. Nos. 1 to 21 (Plate 4), and the tests extended from November 9th to December 8th.

When considering the advantages of any description of insulator, however, it is evident that one may have a better form for insulation under all circumstances than another of an inferior shape, but it may give an equal or lesser absolute resistance, owing to the latter having a greater length, or lesser section than the former. It therefore becomes desirable to reduce the absolute results, obtained in a series of tests such as those recorded, to an unit result, so that a clear conception of the value of form may be obtained, and, if necessary, increased length or diminished section be applied to get the best results. This is done in the table in the following manner. The lengths of each cup are divided by the mean circumference, both inner and outer. The fractions thus obtained are added together giving a final fraction, the numerator of which represents the sum of the lengths, the denominator the sum of the circumferences. Now, as the resistance increases directly as the length and inversely as the circumference, if we divide by the former and multiply by the latter we get a result which would be recorded were all the insulators so made that each exposed precisely the same surface for conduction. This may be termed the form value of the insulator, and if no disturbing elements arise the figures given under this head should accurately represent the advantages and disadvantages of any given form. In the table the fraction—length by circumference—is reduced to a decimal fraction by which each absolute measurement is divisible to obtain the unit or form value.

The results obtained are all tabulated in the same order as they appear in the illustrations in the annexed schedule, which gives the absolute resistance, the resistance reduced to unit length, and the order of merit obtained by each insulator.

1

Date.	State of weather.	Porcelain, D.S. Terminal Insulator. Divisor 1·025.			
		Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
1877					
Nov. 9	Fine - - - -	306·25	18	298·78	19
12	Slight rain - -	77·500	17	75·609	14
13	Damp- - - -	2·411	19	2·352	19
14	Thick fog at night	1·750	19	1·707	19
15	Rain - - - -	3·100	16	3·024	15
16	Fine - - - -	43·255	18	42·200	18
17	Foggy - - -	4·384	17	4·277	19
19	Rain - - - -	1·971	15	1·922	14
20	Rain - - - -	8·1177	10	7·9196	10
21	Rain - - - -	1·914	18	1·6152	12
22	Rain - - - -	5·2978	19	5·1686	19
23	Fine after rain -	24·081	13	23·493	13
24	Rain - - - -	·44896	13	·43801	10
26	Fine - - - -	8·9032	20	8·6861	21
27	Fine - - - -	50·941	11	49·698	7
28	Rain during night	1·8375	21	1·7927	21
29	Rain during night	5·1579	18	5·0321	20
30	Rain during night	5·1786	20	5·0523	20
Dec. 1	Damp fog - -	0·70732	18	6·69007	16
3	Rain - - - -	1·	18	0·97561	20
4	Rain - - - -	·49181	18	·47981	21
5	Foggy - - -	·7500	21	·73171	21
6	Rain - - - -	·66666	17	·65041	20
8	Fine - - - -	1·1111	16	1·084	20
	Mean of 24 results	23·21773	19	22·64076	19
	Highest result -	306·25	19	298·78	19
	Lowest result -	·44896	14	·43801	11
	Mean of 23 results	10·912	20	10·643	20

2				3			
Earthenware, D.S. Varley's. Divisor 1·106.				Porcelain, D.S. P.O. form. Divisor 1·432.			
Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.	Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
1002·3	10	906·21	11	664·16	15	463·79	15
235·869	7	213·263	8	144·666	10	101·023	11
13·072	7	11·873	6	4·245	15	2·964	17
8·480	9	7·667	6	3·006	15	2·099	18
3·576	14	3·233	13	5·936	10	4·145	10
254·794	10	230·374	9	104·670	15	73·093	16
7·015	14	6·342	10	7·892	12	5·511	13
5·073	7	4·586	7	3·798	8	2·652	9
22·258	6	20·125	6	11·829	8	8·2607	9
3·4854	6	3·1514	7	3·4065	7	2·37885	8
9·1341	17	8·2587	17	12·546	14	8·8356	15
77·909	8	70·442	9	74·533	9	52·048	11
1·0347	6	·93555	5	1·1686	5	·81611	6
23·	13	20·796	8	23·488	12	16·402	14
25·471	16	23·029	16	23·371	17	16·324	20
7·1707	11	6·4835	13	5·6298	14	3·9315	14
23·719	14	21·437	16	18·123	16	12·656	19
11·153	15	10·085	14	9·3218	17	6·5096	18
1·5263	10	1·380	10	1·2671	12	0·8848	20
1·9863	13	1·796	18	1·631	16	1·139	19
·9375	19	·84765	14	·8982	20	·62724	19
1·1538	16	1·0433	18	1·1719	15	·81835	19
1·500	15	1·3562	12	1·8726	8	1·3077	14
2·5862	13	2·3383	13	2·0775	15	1·4508	15
72·4668	12	65·521	12	47·112	17	33·3194	16
1002·3	11	906·21	12	664·16	16	463·79	16
1·0347	4	·93555	4	·8982	6	·62724	7
32·040	11	28·9	9	20·284	16	14·168	16

4

Date.	State of Weather.	Earthenware, S.S. P.O. form. Divisor ·881.			
		Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit
1877					
Nov. 9	Fine - - - -	1531·2	9	1738·02	6
12	Slight rain - -	241·111	6	273·678	5
13	Damp - - -	8·037	12	9·122	7
14	Thick fog at night	6·057	12	6·875	8
15	Rain - - - -	2·657	18	3·015	16
16	Fine - - - -	300·000	8	540·522	5
17	Foggy - - -	5·612	15	6·370	9
19	Rain - - - -	1·840	17	2·088	13
20	Rain - - - -	7·5824	12	8·6066	8
21	Rain - - - -	1·1037	17	1·2528	17
22	Rain - - - -	4·4897	20	5·0961	20
23	Fine after rain	16·556	15	18·791	14
24	Rain - - - -	·31584	16	·35794	14
26	Fine - - - -	39·429	7	44·754	7
27	Fine - - - -	33·111	14	37·583	10
28	Rain during night	5·880	15	6·6742	10
29	Rain during night	25·789	13	29·273	15
30	Rain during night	10·985	16	12·468	12
Dec. 1	Damp fog - -	·46774	21	·53092	18
3	Rain - - - -	·58468	20	·66365	21
4	Rain - - - -	·4000	21	·45403	20
5	Foggy - - -	·57252	20	·64985	20
6	Rain - - - -	·76928	16	8·7314	18
8	Fine - - - -	1·0135	19	1·1504	19
	Mean of 24 results	93·5651	10	89·1437	7
	Highest result -	1531·2	10	1738·02	7
	Lowest result -	·31584	16	·35794	14
	Mean of 23 results	31·059	18	17·341	14

5				6			
Porcelain, D.S. Andrew's Old. Divisor 1·354.				Porcelain, D.S. P.O. Large New. Divisor 1·607.			
Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.	Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
755·14	13	557·71	14	12250·	1	7622·9	1
348·874	5	257·661	6	7233·333	1	4501·140	1
16·564	5	12·233	5	657·575	1	409·194	2
9·450	8	6·979	7	229·687	2	142·929	3
13·721	6	10·133	5	1550·000	3	964·530	3
429·561	5	317·253	6	23250·000	1	14467·952	1
24·761	5	18·27	6	876·875	2	545·659	3
7·861	5	5·805	6	4058·823	1	2525·714	1
77·659	5	57·355	5	13800·	1	8587·4	1
4·7693	5	3·5224	6	1655·5	1	1030·2	1
11·92	15	8·8037	16	44148·	2	43541·	2
12·818	16	9·4665	19	132440·	1	82414·4	1
·59601	10	·44018	11	756·83	1	470·96	1
88·717	4	65·523	5	34500·	1	21469·	1
74·533	5	55·046	6	14716·	1	9157·5	1
37·808	5	27·924	6	12250·0	1	7621·0	1
49·00	12	36·189	12	3675·0	1	2286·9	2
48·706	7	35·973	6	11154·0	1	6940·8	1
3·7275	6	2·753	7	966·66	1	601·54	1
8·3815	5	6·1902	5	3020·0	1	1879·8	1
2·5201	7	1·9002	8	555·55	1	345·71	1
6·3291	5	4·6744	4	1119·4	1	696·58	1
3·5294	7	2·6066	8	721·15	1	448·76	1
6·1476	9	4·5403	7	4687·5	1	2916·9	1
85·12981	11	62·85506	18	13828·1	1	9282·961	
755·14	14	557·71	14	132440·	1	82414·4	
·59601	10	·44018	10	555·55	1	354·71	
56·004	5	41·36	5	13374·8	1	9802·9	1

7

Date.	State of weather.	Porcelain, D.S. Schomburg's. Divisor 1·316.			
		Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
1877					
Nov. 9	Fine - - - -	229·69	19	174·54	20
12	Slight rain - -	83·461	16	63·420	17
13	Damp - - -	2·932	17	2·227	20
14	Thick fog at night	2·205	17	1·675	20
15	Rain - - - -	3·381	15	2·569	19
16	Fine - - - -	68·888	17	52·346	17
17	Foggy - - -	4·008	18	3·045	21
19	Rain - - - -	3·209	9	2·438	10
20	Rain - - - -	7·0769	13	5·3776	13
21	Rain - - - -	3·3111	8	2·5160	9
22	Rain - - - -	9·4603	16	7·1887	18
23	Fine after rain -	20·068	14	15·249	15
24	Rain - - - -	·98838	7	·75106	7
26	Fine - - - -	25·091	11	19·006	11
27	Fine - - - -	22·835	18	17·352	19
28	Rain during night	4·8196	16	3·6624	16
29	Rain during night	21·94	15	16·672	17
30	Rain during night	7·1782	18	5·4546	19
Dec. 1	Damp fog - -	1·450	16	1·1018	8
3	Rain - - - -	1·5761	15	1·1976	16
4	Rain - - - -	·87289	16	·66268	18
5	Foggy - - -	1·7647	13	1·3409	14
6	Rain - - - -	1·500	15	1·1398	15
8	Fine - - - -	1·875	17	1·4248	16
	Mean of 24 results	22·06588	20	16·76	20
	Highest result -	229·69	20	174·54	20
	Lowest result -	·87289	7	·66268	6
	Mean of 23 results	13·039	18	9·90	18

8				9			
Porcelain, S.S. Schomburg's. Divisor ·873.				Porcelain, S.S. P.O. Form. Divisor ·667.			
Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.	Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
551·25	17	631·44	13	4·4702	20	6·7466	21
74·315	19	85·126	12	38·070	20	57·076	19
2·523	18	2·890	18	1·446	20	2·107	21
1·868	18	2·139	17	·776	20	1·163	21
2·480	19	2·840	18	1·240	20	1·859	20
100·540	16	115·166	14	7·750	21	11·619	21
4·384	17	5·021	16	2·922	19	4·380	18
1·314	19	1·505	16	·985	21	1·476	17
4·1818	17	4·7902	14	1·84	20	1·7842	20
1·2495	16	1·4312	13	·83826	20	1·2568	16
8·8296	18	10·114	14	1·6556	21	2·4821	21
9·8107	18	11·238	18	3·8954	19	5·8402	21
·26975	17	·38901	13	·19279	19	·28904	15
17·25	17	19·759	10	13·398	18	20·087	9
15·581	19	17·849	18	1·2614	20	1·8911	21
5·9692	12	6·6819	9	1·13009	20	1·9503	20
13·009	17	14·901	18	1·3363	19	2·0036	21
7·250	19	8·3047	17	2·900	21	4·3479	21
·59184	19	·67793	17	·55343	20	·82974	15
1·0140	17	1·1615	17	0·80927	19	1·2077	15
·63291	17	·72498	16	·42857	4	·64253	4
1·	18	1·1455	17	·80214	19	1·2026	15
1·1538	13	1·3217	13	·61983	18	·92929	17
1·0714	18	1·2273	17	·78947	21	1·1836	18
34·48103	18	39·4935	15	3·71888	21	5·55647	21
551·25	18	631·44	13	38·070	21	57·076	21
·26975	17	·38901	13	·19279	19	·28904	15
12·013	19	13·757	17	3·880	21	5·82	21

10

Date.	State of weather.	Porcelain, Iron Capped. Johnson and Phillips' Oil Well.			
		Resistance. Megohms.	Order of Merit.	Reduced. Resistance.	Order of Merit.
1877					
Nov. 9	Fine - - - -	5011·4	2		
12	Slight rain - -	1972·727	2		
13	Damp - - -	630·815	2		
14	Thick fog at night	102·558	4		
15	Rain - - - -	3381·818	1		
16	Fine - - - -	12400·000	2		
17	Foggy - - -	1062·878	1		
19	Rain - - - -	35·384	4		
20	Rain - - - -	290·53	3		
21	Rain - - - -	66·222	3		
22	Rain - - - -	155·81	4		
23	Fine after rain -	3531·9	3		
24	Rain - - - -	19·477	3		
26	Fine - - - -	55·2	6		
27	Fine - - - -	60·202	8		
28	Rain during night	29·40	6		
29	Rain during night	117·6	4		
30	Rain during night	305·26	3		
Dec. 1	Damp fog - -	23·20	3		
3	Rain - - - -		8		
4	Rain - - - -		10		
5	Foggy - - -		10		
6	Rain - - - -		8		
8	Fine - - - -		20		
	Mean of 24 results	1539·6*	3		
	Highest result -	12400·	3		
	Lowest result -	29·4	5		
	Mean of 23 results	1346·6†	3		

* Mean of 19 results. † Mean of 18 results.

11				12			
Porcelain, D.S. Indian Government. Divisor 2·009.				Porcelain, S.S. Andrew's New. Divisor 1·120.			
Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.	Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
861·33	11	428·73	17	3150·	3	2812·5	3
226·083	8	112·535	9	350·000	4	312·500	4
11·421	8	5·684	12	6·200	13	5·535	13
10·400	6	5·176	11	2·689	16	2·400	16
6·305	9	3·138	14	3·720	13	3·321	12
32·631	20	16·242	20	224·096	11	200·085	10
8·017	11	3·990	20	25·519	4	22·775	5
3·066	11	1·526	15	1·189	20	1·061	21
8·9031	11	4·4316	15	3·6316	19	3·2425	19
1·8395	12	·91564	18	·77909	21	·69561	20
55·649	8	27·7	9	21·023	11	18·771	12
12·263	17	6·1041	20	16·556	15	14·782	17
·57585	11	·28663	16	·11037	20	·08954	21
19·714	15	9·813	20	86·25	5	77·009	3
43·425	13	21·615	17	82·778	3	73·909	3
13·363	8	6·6519	11	3·4186	18	3·0524	17
73·50	6	36·585	11	61·25	9	54·687	7
30·417	8	15·036	10	26·363	9	23·539	8
1·9463	7	·9688	19	1·160	17	1·0357	11
4·2647	10	2·1228	14	9·6666	4	8·832	4
1·500	13	7·4664	5	1·200	14	1·0714	11
8·0213	4	3·9927	5	3·7500	8	3·3482	8
3·5294	7	1·7568	9	1·5789	11	1·4098	10
6·8182	8	3·3938	9	15·00	4	13·393	4
60·20759	14	30·24411	18	170·747	5	152·4563	5
861·33	12	428·73	17	3150·	4	2812·5	4
·57585	11	·28663	17	·11037	20	·0985	21
25·376	15	12·919	19	41·214	10	36·802	7

13

Date.	State of Weather.	Porcelain, D.S. Schomburg's. Divisor ·808.			
		Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
1877.					
Nov. 9	Fine - - - -	787·5	12	974·63	10
12	Slight rain - -	90·416	15	111·900	10
13	Damp - - -	3·208	16	3·970	15
14	Thick fog at night	3·242	14	4·012	13
15	Rain - - - -	3·381	15	4·184	9
16	Fine - - - -	143·076	15	177·074	12
17	Foggy - - -	5·101	16	6·313	11
19	Rain - - - -	1·769	18	2·189	12
20	Rain - - - -	5·6327	16	6·9712	11
21	Rain - - - -	1·409	19	1·7438	11
22	Rain - - - -	18·395	12	22·767	11
23	Fine after storm	46·966	10	58·126	10
24	Rain - - - -	·48162	12	·59606	8
26	Fine - - - -	13·143	19	16·266	16
27	Fine - - - -	29·432	15	36·425	11
28	Rain during night	2·3333	19	2·8878	18
29	Rain during night	23·709	14	29·344	14
30	Rain during night	138·09	4	170·91	4
Dec. 1	Damp fog - -	1·2803	15	1·4995	7
3	Rain - - - -	1·8125	14	2·2432	12
4	Rain - - - -	·96774	15	1·1977	10
5	Foggy - - -	1·6216	14	2·0069	11
6	Rain - - - -	1·1363	14	1·4064	11
8	Fine - - - -	2·5000	14	3·094	12
	Mean of 24 results	51·10546	16	63·2493	10
	Highest result -	787·5	13	974·63	11
	Lowest result -	·48162	12	·59606	8
	Mean of 23 results	19·089	17	23·62	12

14				15			
Porcelain, D.S. Fuller's Inverted Cone. Divisor 1·803.				Porcelain, D.S. Corrugated Exterior. Divisor 1·625.			
Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.	Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
648·5	16	359·69	18	1968·8	7	1211·5	8
108·500	13	60·177	18	113·020	12	69·550	16
9·644	11	5·348	14	5·023	14	3·091	16
6·485	11	3·596	14	5·512	13	3·392	15
5·211	11	2·890	17	2·906	17	1·788	21
158·298	13	87·797	15	221·428	12	136·263	13
8·017	11	4·446	17	8·252	10	5·078	15
2·816	12	12·099	5	2·254	14	1·387	19
6·4942	14	3·6018	18	6·2727	15	3·8602	16
2·3034	9	1·2776	15	1·5767	13	9·7029	5
50·455	9	27·984	8	16·555	13	10·188	13
151·36	7	83·952	8	24·527	12	15·093	16
·71592	8	·39707	12	·45988	14	·28562	17
29·052	10	16·114	17	29·362	9	18·069	12
50·455	12	27·984	14	55·185	10	33·96	12
20·631	7	11·443	7	4·1525	17	2·5554	19
68·372	8	37·921	9	51·399	10	31·63	13
17·576	12	9·748	16	24·576	10	15·124	9
1·9333	8	1·0723	9	1·4872	11	·91519	14
4·4615	9	2·4745	11	3·5366	11	2·1764	13
2·0862	9	1·157	13	1·1111	11	·68376	17
3·1983	11	1·7739	12	1·875	17	1·1538	16
·7500	19	·41597	21	1·3513	12	·83100	19
6·0402	10	3·3278	10	5·0000	11	3·0769	11
56·80646	15	31·94529	17	106·48425	9	65·5287	11
648·5	17	359·69	18	1968·8	8	1211·5	9
·71592	8	·39707	12	·45988	13	·28562	16
31·08	12	17·695	13	25·524	14	15·7	15

16

Date.	State of Weather.	Porcelain, D.S. Iron-capped. Divisor 1·207.			
		Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
1877.					
Nov. 9	Fine - - - -	2901·3	4	2403·7	4
12	Slight rain - -	571·052	3	473·116	3
13	Damp - - -	135·625	3	112·365	3
14	Thick fog at night	208·18	3	172·342	2
15	Rain - - - -	64·137	4	53·137	4
16	Fine - - - -	1328·571	4	1100·720	4
17	Foggy - - -	163·139	6	135·160	4
19	Rain - - - -	138·000	2	114·333	2
20	Rain - - - -	345·0	2	285·83	3
21	Rain - - - -	34·854	4	28·876	4
22	Rain - - - -	206·94	3	171·45	3
23	Fine after storm	174·27	5	144·38	5
24	Rain - - - -	5·4729	4	4·5343	4
26	Fine - - - -	92·	3	76·222	5
27	Fine - - - -	79·786	4	66·103	4
28	Rain during night	44·546	4	36·906	4
29	Rain during night	91·875	5	76·119	6
30	Rain during night	60·416	5	50·055	5
Dec. 1	Damp fog - -	10·584	4	8·7688	4
3	Rain - - - -	13·942	3	11·551	3
4	Rain - - - -	7·500	5	6·2138	6
5	Foggy - - -	12·00	3	9·942	3
6	Rain - - - -	15·625	3	12·945	3
8	Fine - - - -	16·666	3	13·808	3
	Mean of 24 results	280·05495	4	232·0240	4
	Highest result -	2901·3	5	2403·7	5
	Lowest result -	5·4729	3	4·5343	3
	Mean of 23 results	166·088	4	137·603	4

17				18			
Porcelain, D.S. P.O. Form. Divisor 1·432.				Porcelain, D.S. Terminal. Divisor 1·341.			
Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.	Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
1696·1	8	1184·5	9	2450·	5	1827·	5
76·140	18	53·170	20	92·735	14	69·1536	15
9·863	10	6·887	10	9·863	10	7·354	9
14·225	5	9·933	5	6·562	10	4·893	12
13·777	5	9·620	6	5·166	12	3·852	11
265·714	9	185·554	11	357·692	7	266·735	7
7·482	13	5·224	14	8·768	9	6·538	8
1·943	16	1·356	20	3·136	10	2·338	11
9·2000	9	6·4246	12	17·250	7	12·864	7
1·8786	11	1·3119	14	2·547	10	1·8994	10
99·995	5	69·803	5	57·585	7	42·941	7
46·068	11	32·170	12	509·41	4	379·87	4
·39243	15	·27404	18	·66222	9	·49383	9
20·072	14	14·017	19	19·166	16	14·292	18
66·223	6	46·245	8	33·111	14	24·692	15
84·00	3	58·659	3	9·1875	10	8·6252	8
168·0	2	117·32	3	49·83	11	37·159	10
46·40	6	32·402	7	13·809	14	10·298	13
1·2185	14	·8509	21	1·2609	13	·94025	13
4·4615	9	3·1156	8	3·3721	12	2·5146	10
2·2865	8	1·5968	9	1·0949	12	·81647	15
3·4325	9	2·397	10	2·0134	12	1·5014	13
10·949	4	7·6459	4	5·2265	6	3·8975	6
12·00	5	8·3799	5	2·8846	12	2·1021	14
110·9092	8	77·45336	9	152·59717	6	113·86543	6
1696·	9	1184·	10	2450·	6	1827·	6
·39243	15	·27404	18	·66222	9	·49383	9
41·988	8	29·32	8	52·71	6	39·381	6

19

Date.	State of Weather.	Porcelain, D.S. Fuller's and Langdon's. Divisor 1·791.			
		Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
1877					
Nov. 9	Fine - - - -	2396·7	6	1338·2	7
12	Slight rain - -	135·625	11	75·725	13
13	Damp - - -	14·276	6	7·970	8
14	Thick fog at night	10·400	6	5·806	10
15	Rain - - - -	10·333	7	5·769	8
16	Fine - - - -	422·727	6	236·028	8
17	Foggy - - -	10·021	8	5·595	12
19	Rain - - - -	6·000	6	3·350	8
20	Rain - - - -	4·1818	18	2·3349	21
21	Rain - - - -	1·3245	15	·7395	21
22	Rain - - - -	47·987	10	26·793	10
23	Fine after rain -	165·56	5	92·437	7
24	Rain - - - -	·31534	16	·17607	19
26	Fine - - - -	30·666	8	17·123	13
27	Fine - - - -	57·585	9	32·153	13
28	Rain during night	11·855	9	6·6191	12
29	Rain during night	70·00	7	39·084	8
30	Rain during night	17·262	13	9·6381	15
Dec. 1	Damp fog - -	4·6774	5	2·6116	5
3	Rain - - - -	7·6316	6	4·2611	7
4	Rain - - - -	3·000	6	1·675	12
5	Foggy - - -	4·4117	7	2·4633	9
6	Rain - - - -	1·8182	10	1·0152	16
8	Fine - - - -	7·1429	7	3·9882	8
	Mean of 24 results	143·39585	7	80·06479	8
	Highest result -	2396·7	7	1338·2	8
	Lowest result -	·31534	16	·17607	19
	Mean of 23 results	45·426	7	25·363	11

20				21			
Porcelain. Prussian Form. Divisor 1·593.				Porcelain. Johnson and Phillip's Oil Well. No. 5.			
Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.	Resistance. Megohms.	Order of Merit.	Reduced Resistance.	Order of Merit.
735·	14	461·4	16	664·16	15		
77·500	17	48·65	21	221·428	9		
10·046	9	6·306	11	27·125	4		
9·843	7	6·178	9	7350·000	1		
9·300	8	5·838	7	2480·000	2		
37·200	19	23·352	19	2480·000	3		
14·030	7	8·807	7	8252·941	3		
2·300	13	1·443	18	54·653	3		
5·75	17	3·7362	17	198·56	4		
1·3941	14	·90588	19	178·5	2		
66·222	6	43·029	6	52978·0	1		
165·56	5	107·57	6	26489·	2		
·25980	18	·16874	20	176·59	2		
25·091	11	16·303	15	13800·	2		
60·754	7	39·476	9	5297·8	2		
5·6538	13	3·6737	15	420·0	2		
147·0	3	95·516	5	3675·0	1		
22·308	11	14·495	11	2230·8	2		
1·5264	9	·99176	12	243·7	2		
5·8468	7	3·799	9	21·481	2		
272·72	2	177·21	2	64·935	3		
5·4545	6	3·5442	6	21·739	2		
8·3333	5	5·4148	5	162·16	2		
7·8947	6	5·1298	6	34·325	2		
70·5411	13	44·281	14	5313·458	2		
735·	15	461·4	15	52978·	2		
·25980	18	·16874	20	21·481	2		
41·651	9	26·14	10	5428·6	2		

Now, in considering the mass of figures given in the foregoing table, it becomes necessary to arrive in the first place at some definite basis of comparison. Evidently the highest result is not fitted for this, nor should the lowest be definitively accepted, as it might be due to some exceptional cause, independent of the general value of the insulator. The mean of the two extremes in most cases would give the same relative value as the highest, and the mean of all the observations, even, is affected very considerably by the first results, which are very unstable, for it is one of the noticeable features in these figures that not only are the original resistances never again reached, in the ordinary forms of insulators, but the nearest approach, after a few days' exposure, varies from one-fourth to one-tenth only. It appears, therefore, that the fairest results will be obtained by rejecting the first readings and taking the mean of the 23 following ones. This has been done accordingly, and although the mean of all the readings, together with the two extremes, are given in addition at the bottom of the large table, the average of the 23 readings is alone taken as the basis of comparison. The insulators are accordingly re-arranged according to their resistances in the next table for more ready consideration.

AVERAGE OF 23 TESTS OF 10 INSULATORS OF EACH DESCRIPTION.

No. on pole and in figures.	Description.	Average Resistance, absolute.	Order of Merit.	Reduced Resistance.	Order of Merit.
6	Porcelain D.S. P.O. large New	13374·8	1	9302·9	1
21	Johnson's and Phillip's No. 5	5428·6	2	5428·6	2
10	Johnson's and Phillips' Iron-capped ...	1346·6	3	1346·6	3
16	Porcelain D.S. Iron-capped	166·088	4	137·603	4
5	Porcelain D.S. Andrew's Old	56·004	5	41·36	5
18	Porcelain D.S. P.O. Terminal	52·71	6	39·381	6
19	Porcelain D.S. Fuller's and Langdon's...	45·426	7	25·363	11
17	Porcelain D.S. P.O. Form	41·988	8	29·32	8
20	Porcelain D.S. Prussian Form	41·651	9	26·14	10
12	Porcelain D.S. Andrew's New	41·214	10	36·802	7
2	Earthenware D.S. Varley's	32·040	11	28·9	9
14	Porcelain D.S. Fuller's Cone	31·08	12	17·695	13
4	Earthenware S.S. P.O. Form	31·059	13	17·341	14
15	Porcelain D.S. Corrugated	25·524	14	15·70	15
11	Porcelain D.S. Indian Form	25·376	15	12·919	19
3	Porcelain D.S. P.O. Form	20·284	16	14·168	16
13	Porcelain D.S. Schomburg's Short ...	19·089	17	23·62	12
7	Porcelain D.S. Schomburg's Varley's Form	13·039	18	9·90	18
8	Porcelain S.S. Schomburg's Varley's Form	12·013	19	13·757	17
1	Porcelain D.S. Terminal...	10·912	20	10·648	20
9	Porcelain S.S. P.O. form...	8·880	21	5·82	21

Now, at the head of the list appears No. 6, the new Post Office large insulator, and perhaps the most salient point in the whole of these tests is the extraordinary resistance recorded by it. This result does not arise from its sectional dimensions, for nothing approaching the same figures is recorded by others with a higher unit division. Such a difference cannot be due to the mere modification of form, and the author has scarcely arrived at a satisfactory conclusion as to the cause. It at first appeared as though the india-rubber ring, interposed between the bolt and the cup, took up the role of the oil in Messrs. Johnson and Phillips's insulators; it being placed out of reach of the direct action of rain, and having little affinity for the ordinary suspended moisture of the atmosphere. Further experiments, however, with other specimens have not given such high results, so that for the present the cause is somewhat obscure.

Next in order appears Messrs. Johnson's and Phillips' insulator. The tests taken at Bristol do not record such high results as those obtained by the inventors. Possibly this may be due to some defect in the oil employed in the experiments; and doubtless the inventors themselves can give a good account to the Society of what they have achieved. They undoubtedly deserve the thanks of the profession for having stepped out of the groove and designed a new method of arriving at a much desired end.

The perforated iron-capped insulator comes next in order, after the oil well form. The result doubtless arises from the protective action from the effects of dew, &c., which the iron cap exercises. It is well known that solid iron caps eventually exercise a very prejudicial effect on insulators by fostering accumulation of dust, &c.; but the perforated ones appear free from these defects for a period, though doubtless a time would arrive when they would reach the level of other insulators of a similar class without the iron hoods.

Now, with reference to the remainder of the insulators on the table, much might be said no doubt by those directly interested in their production on the advantages which each form may possess. There is, further, little doubt that some of them do not occupy their right places in the order of merit on the last table, nor do they

stand so high as they might or should have done had their surfaces been chemically clean and uniformly covered with a thin layer of moisture. Such irregularities are certainly due to some extraneous causes not visible on ordinary inspection. But this fact suggests matter for serious reflection. When it is observed that, for instance, an earthenware single shed insulator takes a higher rank than a high class expensive porcelain one, which latter must under normal conditions give a far higher resistance than the former,—when again it is found that of two lots of insulators of the same pattern, and of the same porcelain, one stands No. 8 on the list, the other No. 16, with only half the average resistance,—and when further it is stated that although no extraordinary precautions were taken to insure chemically clean surfaces, for a test under practical conditions was aimed at, yet the whole of the insulators tested were handled with far more care than is possible, or is ever devoted to them in line construction,—it becomes a serious question whether attention should not be turned in a different direction in endeavouring to effect future improvements. No doubt in the long run the costly large double-cup insulator, with a high unit divisor, must show its superiority over the single cup with its low divisor ; but do we get the value of the extra expenditure under a system which admits of this expenditure being, to a great extent, nullified by accidental circumstances which in some cases cannot be detected on inspection? Further, after a greater or lesser period of exposure, however perfect the result recorded at first may be, a time arrives when under unfavourable climatic circumstances, rapid and delicate working becomes difficult on long wires, even with the best of ordinary insulators. No doubt great results have been achieved with existing systems of insulation, but if the introduction of quadruplex has not quite brought us to the practical limit of improvement in the transmission of signals, with our present minimum insulation, it is probable that we are closely approaching it; and who can say, after the invention of the telephone, what other discoveries, which would revolutionise signalling, may be close at hand, but which would find us unprepared to work them, consequent on the low insulation and variability of our lines; in the same manner as the duplex system was found im-

practicable, when first designed, from precisely similar causes? Of course it is not urged that, in view of what some may consider problematical improvements, great and immediate expenditure should be incurred and radical changes should be introduced; but that the time is approaching, if it has not actually arrived, when another step forward should be made, and that we should be prepared to make that step. It is thought that one direction in which great improvement is possible is in the provision of an unchangeable section in an insulator, placed as far as possible beyond the reach of all accidental causes of interruption or deterioration, and independent to a great extent of climatic changes. It has already been shown how this problem may in one way be solved; doubtless other means can be designed if needed.

The CHAIRMAN: We have listened to a very carefully drawn up paper on a subject of vast importance to every telegraph engineer. I am afraid we were not able to appreciate the value of the paper fully, because it depends so much on a number of tables which the author was unable to furnish us in a form and on a scale to exhibit to the meeting, but they will of course be published with the paper. There are many gentlemen present who would no doubt like to say a few words on the subject, who have paid special attention to the manufacture of insulators of different forms. I will call upon any volunteers from amongst the representatives of the insulators referred to this evening to favour us with remarks. Messrs. Johnson and Phillips are both here, and we shall be glad to hear them, or any other gentleman, on this subject.

Mr. S. E. PHILLIPS said: It is more than three years ago since I conceived the idea of using oil as an insulator on our land lines. The results I arrived at at an early period made me feel very sanguine, and after three years' experience I am more than ever convinced that it was a step in the right direction. Mr. Gavey has described briefly the principle of my insulator: it depends upon the interposition between "line" and "earth" of a fluid insulator which will not support a film of dust or moisture upon its surface.

It is evident that the idea may be applied in a great number of ways, and we have already made a good many different forms;

this, however, is a point Mr. Johnson will be able to deal with more clearly than I can.

There are one or two points I would refer to in the results given by Mr. Gavey. Curiously enough, after pointing out that nearly all insulators which have only a solid surface between "line" and "earth," and which must in course of time give great surface leakage, he heads his list with an insulator of that character; he, however, points out that the result is somewhat a mystery to him, and I am also at a loss to understand it, except that it may in some measure be due to the india-rubber ring, which it is suggested will not condense moisture upon its surface. We, however, all know that india-rubber very soon becomes conducting on its surface, and as the test was only for the period of a month it is possible that it was merely a hygroscopic value of a clean insulator that was tested. Those of ours which we have had up over thirteen months are perfectly black and dirty, and it would be interesting to know the condition of those tested by Mr. Gavey in this respect. There is a very anomalous feature in the test of our insulator, which takes the second place in Mr. Gavey's list. The first test, which was made on a fine day, gave 664 megohms; the second, 221; on the next day, 27; and on the following day, *in rain*, it rose to 7,350 megohms. This is a very remarkable result, and I regret Mr. Gavey is not here, since he might be prepared to discuss the point or give some information as to the way in which the values were ascertained. The lowest he has is 21 megohms—on the No. 5 * insulator. It is an unfortunate thing that while our insulator was in the hands of a perfectly impartial investigator like Mr. Gavey it should have given results so much below our own. My results have, however, been confirmed by several gentlemen who have been to our works and tested the 20 No. 5. I wish Mr. Gavey had also been and seen a test taken. We however hope he may still do so. I think he has hit upon the true explanation of the difference between our results, viz., that the oil is in fault. We sent Mr. Gavey some samples of our insulators, but unfortunately did not send the oil as well—had we done so I am sure his results would have corresponded more nearly with my own. I had no

* No. 21 in illustrations.

idea that the kind of oil was so important, and I wrote Mr. Gavey some time ago to say we had used rosin oil with wonderful results, but that I thought almost any oil would do. I have myself since, with different oils, got results as low as those of Mr. Gavey. 20 No. 5 insulators, which were put up on the 5th of January, 1877, still give an absolute resistance of between 1,000 and 2,000 megohms, and on testing them yesterday morning in a fine rain, I obtained these results:—"20 No. 5, absolute resistance, 1,803 megohms; 20 ordinary insulators, ·1 megohm. That is to say, our 20 No. 5s' were 18,030 times better than the ordinary ones, after having been fixed in an exposed position without any attention for fourteen months.

The other form which Mr. Gavey tested is an iron-sheathed insulator. Insulators with iron heads have been mostly abandoned because of the dirt they allow to accumulate on the porcelain, but it was evident in our case it did not matter how dirty the porcelain became; therefore we may use the iron sheath with impunity. I have no doubt this form will give good results when charged with the proper kind of oil.

We have been asked a good many questions as to how long the oil will last, and whether it will answer in hot climates, but that is a question which nothing but extended trial can definitely settle. I can only say in an insulator which we have had up since January the 15th last year the oil seems as perfect as on the day it was put in, and is not, as far as can be detected, at all diminished in bulk. Anticipating this question I partly filled a glass beaker with oil on the 10th of November, 1876; I examined it carefully yesterday morning, and there was scarcely any perceptible diminution in the bulk, and its surface was perfectly clear and bright. The oil, which we hit upon by a mere accident, is clearly a very good one for the purpose. I was noticing the other day two large pans outside our laboratory—one containing rosin oil and the other Stockholm tar. As the result of the men sweeping the dusty passage every morning the surface of the tar was thick with dust, whilst the pan of rosin oil retained a surface like a mirror. With reference to warm climates like that of India, I believe this oil will last a very long time without requiring renewal. We have three or four insulators fixed close to the top of an iron chimney shaft. One was charged with oil, and

they have been up about six months. The heat was so intense as in one case to melt the sulphur cement, notwithstanding which I found the oil in perfect condition. [*The position of the insulators and the nature of the shaft were illustrated on the black board by Mr. Johnson.*] I think until the paper containing the tables referred to has been in the hands of the Members it is impossible properly to enter more fully into the discussion of this paper.

A MEMBER asked with regard to the iron-headed insulator whether Mr. Phillips ever found them bridged over with cobwebs. There was but a small space between the iron head and the cup containing the oil, and these being in close proximity to each other there was a liability of its being bridged over with cobwebs.

Mr. PHILLIPS: With reference to the close contiguity of the cup to the iron head Mr. Graves and Mr. Preece had called our attention to this, and they thought we ought to give a little more space there. I have had some of these insulators up for some months, and none of them have been bridged over in the way mentioned, and it is remarkable what a little leakage occurs from this cause; for instance, in April last I have the following note:

"20 No. 5 insulators give 30 divisions loss; examined them and found spiders' webs connecting the wire to the tree; remove spiders' webs and get 1·5 divisions; 30 divisions = 606 megohms.

"May 11th. On first testing the 20 No. 5 insulators they gave a loss of 3 divisions; on examination I found a spider's web connecting the wire to the trees; on breaking this the loss fell to ·5 division, very fine rain, wind south.

"On Sept. 8th, 1877, I obtained 22 divisions loss on the 20 No. 5's. There were several webs connecting the insulators to the arms, and it was a damp and misty morning: 22 divisions = 807 megohms."

Mr. W. H. PREECE. It was not my intention to say anything on the subject of insulators. My name has been mentioned once or twice, and many of the experiments Mr. Gavey has carried out have been carried out by my instruction. Mr. Gavey himself was not able to be present to-night, but it is very probable he may be here on the next occasion. In the meantime copies of the paper will be distributed to the Members, and I have no doubt all will be able to acquire a larger knowledge of the subject than they have

now, and will be prompted to ask questions, and seek for further information than they now possess. There is no subject which occasions so much thought and trouble to telegraph engineers as that of insulation. It is their *bête noir.* Their first thought in the morning is how the line will work during the day; their last thought at night is how it will work on the following day. Their chief thought (whether the lines under their charge be overground underground, or submarine) is of insulation.

However, to-night we simply deal with the question of aerial insulation, connected with the earliest days of the telegraph. The first telegraph line ever laid down was that from Euston to Camden Town, where the wires were laid underground. The next was from Paddington to West Drayton, which also passed originally underground; but it was soon found that the wires did not work at all well, and they were speedily transferred from underground to a more airy position above. The first insulator ever employed was, I am told, a goose-quill. However, of course, as you may imagine, a goose-quill was not of much use for overground work, and that was replaced by a simple ring—a flat disc of brown earthenware. That, too, was found to work badly, and it was replaced by the iron cone of Cooke and Wheatstone. From the cone many experiments were made by Hatcher, Physick, Little, and others, whose names, although they are living now, are almost forgotten in the telegraph world.

But one great stride in insulation was made when Mr. Edwin Clark came from his successes at the Britannia Tubular Bridge to assume the command of the engineering department of the Electric Telegraph Company, where he and his talented brother, whom we so often have the pleasure of seeing amongst us, devoted a great deal of their attention to this matter, and struck out a completely new path. Mr. Edwin Clark took his idea from the cap that covers an ordinary night-glass. We all know, who have been at sea, that night-glasses are provided with caps that extend over the object-glass, with a view of preventing radiation from the glass, and so preventing the deposition of moisture upon it.

Mr. Clark argued that if the earthenware surface of an insulator were protected with a metal cap, radiation would be checked and

the deposition of moisture would cease; and experiment fully proved the truth of that idea. An insulator was made on that principle. It was of the suspended form, one of which I do not see here. It was suspended with a cap something like an umbrella, made of zinc; and it was found when these insulators were new, during the early morning and late evening, when dew falls, the inside of this metal cap was a perfectly dry zone, and insulation for a few months proved to be no longer a source of trouble. The lines worked with marvellous perfection, distance ceased to be a trouble, and the problem of insulation seemed to have been solved; but time, which destroys many of our fond notions, also destroyed the truth of this very pretty theory. It was found that dust and dirt and soot and other troubles were deposited upon the insulators, and after a short time they became as bad as ever. Then, by replacing the zinc cap by an ordinary earthenware cap, and making the insulator of another form, a great step was taken. Then Mr. Latimer Clark brought out his invert.

There is not one here of the actual form. The insulating surface being protected against the climatic effects of rain and wind, it succeeded to a certain extent in giving us again a prospect of perfect working lines; but here again the little item of time crept in, and the repetition of these dirty troubles showed that we had not arrived at perfection yet. Of course others tried to improve upon that. Amongst others Mr. Varley came to the front, and, by trying an improvement on what had been done by Mr. Clark, took another step—merely making the insulator in the form of a double cup in two pieces. The invert of Mr. Clark took the form of a double cup. Mr. Varley conceived the idea of making the inner cup distinct and separate from the outer cup, assuming that it was almost impossible that two bad cups should be inserted together, and if one was faulty it would be remedied by the perfection of the other. This double-cup insulator of Mr. Varley is a good one. It has held its own for some years, and as an insulator in that form is nearly as good as any insulator used in other countries. Other countries also started on fresh lines, and tried various improvements. The Indian department especially, by improving upon the Prussian model, by reducing the surface and increasing the area,

succeeded in producing an insulator that is in itself a very excellent one; and Mr. John Fuller, assisted by Mr. Langdon, working almost on the same model, struck in another direction, altering the form from this double cup to this conical form. Mr. Andrews had also previously started in another line, by widening the inner cup, extending it down the stem, working upon the idea that the insulation of a cylinder increases directly as its length and inversely as its breadth; and going further, Mr. Andrews departed from the cup-form, and produced this rod-like insulator which you see here. If you could make this rod infinitely thin, it would be a perfect insulator, but as you cannot have strength without thickness so it is impossible to make an insulator of this kind a perfect one.

Another step has been taken by the introduction of this corrugated surface. Mr. Latimer Clark introduced a corrugated surface in the interior of the cup, but Mr. Langdon conceived the notion of placing the corrugation on the exterior, the assumption being that where you have a surface broken into lines those fine surfaces would speedily get dry when rain ceased. The test of this form of insulator is very satisfactory.

No great step in a distinct line however was taken until Messrs. Phillips and Johnson brought out their new form, and I feel bound to confess that they have started on a thorough new basis, which has caused telegraph engineers a good deal of thought, and they have done something which certainly justifies the consideration which is now being given to the subject. At present it is a child. It has not been practically tried on a large scale. It is now being tried. The Post Office recognised at once that these gentlemen had started a fresh field, and we have done all in our power to assist them in carrying out this idea. Mr. Gavey has tested this insulator with the results shown in the paper. At present we have carried them from Westbourne Park to Uxbridge, and, in order to afford a satisfactory test, eight wires have been re-insulated with different forms of insulators. They have all been put up at the same time, and include those of Varley, Clark, Langdon, Fuller, Jobson, and Johnson and Phillips, and, should it happen that between now and the next time we meet our friend Aquarius should favour

us with some wet weather, I hope we may be able to give some practical results of the testing of these various insulators. I look for great results from the latter. The tests shown by Mr. Gavey are anomalous and remarkable; anomalous because they are so variable, remarkable because the results were so far less than those obtained by the inventors, just as the tests of the Post Office form of insulator are anomalous and remarkable in the same degree as that of Messrs Phillips and Johnson. It is probably owing to the india-rubber washer inside here performing the same function as the oil in Messrs. Phillips and Johnson's insulator. My own impression is, that although this little india-rubber washer may perform its function for a month it may not do so for two months. It is a question of time, and I shall expect to find in two or three months when these insulators are tested, as in every previous case, when they get coated with dust and dirt, they will all have become similar. It is because in this pattern of Johnson and Phillips there is an unvarying section always opposing the same surface to the current that we may hope for success. If dirt gets in we must always look for the same unfavourable results. Insulators which have been up for fifteen months have given very favourable results, and if those of Johnson and Phillips succeed in giving good results for twelve months they will certainly have done a wonderful action. Insulation is a matter of such importance that I look forward with great interest and some degree of sanguineness to the success of this invention of Johnson and Phillips. Next time I hope Mr. Gavey will be present to give the results of further experiments, and I am sure you will be interested in hearing his results, whether they are successful or not.

The Chairman: I think we must all admit that this paper cannot be fairly discussed until the tables attached to it have been in our possession, and until we have been able to master them thoroughly. We will, therefore, with your permission, adjourn the further discussion till the next meeting, and I will now only allude as briefly as possible to one or two points which I think are not touched upon in the paper. It seems to me that in constructing a telegraph line, engineers pay great attention to the strength of the insulator stalks and brackets as well as of the posts employed and

to the strain on the wire, but they do not always remember the great strain which is brought upon the porcelain itself, in consequence of which imperceptible cracks take place after the line has been put up and tested. There is another thing with regard to the use of iron coverings to insulators. We have been obliged to use them in Persia, where the inhabitants are rather inclined to mischief; but I believe in climates subject to great changes from heat to cold the contraction and expansion of the metal causes the porcelain to crack, the effect of course being most damaging to the insulation of the line. These matters do not seem to have been treated of in the paper, and might perhaps be considered a little more at the next meeting. As to the form of insulator, I think we must all agree that the fewer separate parts there are the better, and in consequence the No. 5* insulator of Messrs. Johnson and Phillips seems to me preferable, generally speaking, to other shapes where screws and washers are necessary, those parts being liable to injury and likely to be mislaid or lost where they have to be carried long distances. I therefore think the No. 5 insulator is a most convenient form in that respect. The Indian Government have ordered some for experiment, to be used along the coast of Belochistan, between the Persian Gulf and India, a very trying country, exposed to the influences of sea-air, sandstorms, and great changes of temperature. I am inclined to believe that if these insulators answer there, they will really answer anywhere.

The discussion was then adjourned.

The following Candidates were balloted for and declared to be duly elected:—

As Foreign Member:

Mr. Bourne, Montreal.

As Members:

Captain R. C. Mayne, R.M., C.B.	Mr. S. F. Josephs.
Mr. H. Fanshawe.	Mr. J. H. Lane.
Mr. S. H. C. Hutchinson.	Mr. R. B. Flindell.
Mr. A. B. Larkins.	Mr. C. E. Pitman.
Major R. M. Smith, R.E.	Mr. H. P. Owen.

* Fig. 21 in illustrations.

Sir William Young, Bart.
Mr. J. W. B. Duthy.
Mr. C. Duffin.
Mr. W. B. Melville.
Mr. A. D. Hill.
Mr. C. H. Maclean.
Mr. W. J. Browne.
Mr. C. Nigel Jones.
Mr. C. B. D. Marks.
Mr. J. M. Rutherford.
Mr. L. V. Fraser.
Mr. A. R. Ward.
Mr. T. C. Hill.
Mr. G. J. Hare.
Mr. E. R. McGrath.
Mr. W. C. Darling.
Mr. E. O. Walker.
Mr. R. W. P. Adams.
Mr. J. Ovens.
Mr. J. Peake.
Mr. Edward B. Bright.
Mr. T. H. Wells.
Mr. A. J. M. Reade.

As Associates:

Lieut. E. P. Gallwey, R.N.
Mr. Jas. Anderson.
Mr. H. W. Sullivan.
Mr. H. Gray.
Mr. H. Bull.
Mr. T. Hackshaw.
Mr. E. R. Barker.
Mr. W. L. Scott.
Mr. W. Clarke.
Mr. E. Graves.
Mr. R. Matthias.
Mr. C. Blackmore.
Mr. T. W. Lapham.
Mr. W. Hardwick.
Mr. A. Smith.
Mr. J. Williams.
Mr. F. Duberly.
Mr. Harold Imray.
Mr. J. H. Scott.
Mr. John Henry Draper.
Mr. Henry Edmunds, junr.

The Meeting then adjourned.

The Sixty-sixth Ordinary General Meeting was held on Wednesday the 27th of March, 1878, Mr. W. H. Preece, Vice-President, in the Chair.

The Chairman: We will now resume the discussion on Mr. Gavey's Paper on Insulators for Aerial Telegraph Lines, read at the last Meeting, and I will call upon Mr. C. V. Walker to favour us with his remarks.

Mr. C. V. Walker: I have but few remarks to make upon this instructive paper. The part I have taken in insulators is very small—very limited. For four or five years during the early history of the South Eastern telegraph lines, which were erected as far back as 1845 by Mr. (now Sir W. Fothergill) Cooke, the insulators were barrel-shaped, of brown stone-ware, pierced with a tubular hole, and with which telegraph engineers are even now more or less familiar. I soon found that the insulation in those days was very unsatisfactory between London and Dover. We had, for instance, no gutta-percha-covered wires in the many tunnels, most of which were very wet. The insulation employed therein was even the same as with open wires, namely, No. 8 B. W.G., suspended upon the barrel-shaped insulators, clipped on oak arms that were fastened to the tunnel by wall-eyes in the usual way. The step which I took under these circumstances was but one solitary step in advance, and was taken not with a view of introducing a perfect insulator, which would have required other and heavier charges. My form of insulator, the double cone, was but an expansion of the Cooke barrel, and the only alteration on the poles that was required for its introduction was an alteration or enlargement of the clips for attaching the cones to the arms. I would not venture further. We still largely, or for the most part, use this double cone, and get from it very respectable insulation. Ours is

Double-Cone Insulator.

not a long telegraph line; the greatest length was a little over 100 miles, since reduced, from London to Deal, where the time-ball is successfully dropped daily at 1 p.m., along this insulation.* The great feature about insulators, especially on Railways, that lead, as in our case, from London, and go some few miles through its smoke-laden atmosphere, is, that they should have the least possible attraction for smoke and soot. I have always a gang, or more than one gang, of men at work washing insulators, and no sooner do they get to the end of a length than they have almost to begin it again. There is, in fact, in the London district, a perpetual washing of insulators going on, clearing them from the soot and dust that gets attached to them. I have no doubt there are gentlemen present who will have more to tell you of their experience with the, in some respects, complicated forms of insulators which have come into use on the long lines they maintain, and on which the delicate operations of modern days with regard to duplex and quadruplex telegraphy have also been introduced.

Mr. Latimer Clark: I will venture to add a few remarks to those which Mr. Preece and Mr. Walker have made on the subject of the history of insulators, especially as regards those of the Electric Telegraph Company. The earliest form of insulator I have seen was the original quill used on the Great Western line; and I am happy to see with us to night Mr. Greener, who has had a longer experience in these matters than any one. I believe he was connected with the erection of the original telegraph on the Great Western line, and subsequently with that on the Blackwall Railway, and I hope to hear some remarks from him on the subject.

The next form I saw was the earthenware cone which Mr. Walker has termed the barrel-insulator, and which was much

* I find, on reference to my office papers, that this insulator was introduced upon the Red Hill and Reading Branch, the first line of telegraphs which the South Eastern Railway Company themselves erected. The first order for this pattern was given on April 4, 1850, to Doulton, of Lambeth, to be made in his brown stone earthenware. On August 27, 1850, Mrs. Walker dug the first turf of the first hole for the first pole of the above branch at Red Hill Junction, on which pole this form was then first used. On November 22, 1852, the original order for the same form in porcelain was sent to M.M. André, Pillivuyt, and Co., of Foëcy, Cher, France. Their first consignment came to hand on January 31, 1853, which is, I think, the first introduction into this country of porcelain as an insulator for telegraph wires. It was used in France, where I first saw it.—C.V.W.

used by Cooke and Wheatstone. At the date of my first connection with telegraphs, in the year 1850, I saw at Liverpool some insulators which were made by Mr. Hatcher, which I mention, because they had a cup turned up within exactly like the interior of the Johnson and Phillips' insulator. I do not quite know the motive for its being thus turned up; it was probably either to increase the conducting surface or to keep out rain, but the idea struck me as being good. There were only a few miles of these near the Liverpool Tunnel, and if they are still in existence it would be interesting to preserve a specimen of them.

Mr. Edwin Clark, as Mr. Preece stated the other evening, became the engineer of the Electric Telegraph Company at this period, and he introduced the insulator which was known as the dew-cap insulator, consisting of a stem of brown earthenware, protected by a cap of zinc, suspended from oak arms by an iron bolt. That worked wonderfully at the time, and we supposed that all difficulties were overcome, but at length it began to share the fate of all insulators, and to show signs of weakness, owing to the accumulation of smoke and dirt.

I then introduced a similar form of insulator, but instead of the zinc cap I made the bell of the insulator as well as the stem of solid earthenware in one piece. That again seemed to answer wonderfully, and we thought again that we had arrived at finality. That form of insulator was also made of glass in a solid piece. Instead of having an iron bolt cemented into it, which often caused the glass to split, a good many were made with a screw stem cast on the glass, which screwed into a corresponding screw thread in the wooden arm. About that period I became myself the engineer of the Electric Telegraph Company, and very soon turned my attention to insulation, which in this climate is always a troublesome question. Mr. Gavey is good enough to say in his paper that my double-bell insulator, or *invert*, as it is called, has proved the parent of most of the forms which have since come so extensively into use. Now, the fact is that my invert had itself a parent. The first idea was derived by me from Brett and Littles' insulator, a few of which were once in use on the Whitehaven and Furness Railway. It was a single bell, not a double bell, with an

iron stem of the usual form bolted on to the post, and that is the real parent, I consider, of all those inverted form of insulators now so extensively used. Taking that as a model, I began to cast about me to see how it could be improved, and I am happy to say that even in those days we were following much the same guiding principles as those described by Mr. Gavey in his paper. We were aware that the increased length of conducting surface gave a greater resistance; we were aware also that it was important to diminish the diameter of the insulator. This insulator was discussed before all the officers of the Company before it was finally decided upon. The points we had in view were: first, to have a small bearing surface for the wire. At that period I had learnt from Mr. Walker that the insulator which he had described—that is, the double cone, narrow in the centre, with the two cones opening outwards—was doing extremely good work, and on considering it thoroughly I became convinced that its merits could not be entirely due to its form, for the two cones gave two paths of escape for the electric current instead of one. I was more inclined to attribute its merits to the extremely small surface of the wire which touched the insulator. Previous to that time we were in the habit of binding the wire to the insulator by surrounding it with wire, whereby a large conducting surface was brought into contact with the wet and dirt.

In Mr. Walker's insulator the wire only rested upon a small spot, therefore one of the points I determined to introduce in the new insulator was that of having a small bearing surface; the top of the insulator was therefore formed of two hooks crossing each other which prevented the wire from falling out, and the wire rested on a blunt bridge between them.

The next point I tried to obtain was a long distance for the electricity to travel. Pondering over the methods of effecting this, the idea of a double bell gradually presented itself to me. My first thought was to have an insulator exactly like Mr. Andrew's and some others, in which the internal bell was united in solid contact with the iron rod by cement; but I saw that by making the inner bell independent of the iron rod and not in contact with it I could increase the length of surface for the conduction of the elec-

tric current. I was so pleased with the idea that when I patented this insulator on behalf of the Electric Telegraph Company I did not even describe or mention the Andrew's form, as I considered it so inferior to the double bell. I further increased the distance the current had to travel by corrugating both the inside and the outside surfaces of the inner bell, and there was another supposed advantage in that. We imagined that the electricity would not travel so easily over the sharp edge of the corrugation formed by each ring, and that each sharp edge would dry quickly and would form a sort of stop to the electricity. I still further added to the distance the current had to travel by coating the iron pin of the insulator with porcelain enamel or with shellac, and by all these means I gave the electric current a distance of some 16 or 18 inches to travel before it could reach the iron arm. Another point I wished to gain was to make an insulation which could be removed, cleaned, or changed with great facility. I therefore employed cast malleable *iron arms* carrying a conical socket, and by means of the double porcelain hook it was easy to disengâge the wire from the insulator and replace it with a clean one. My idea was, that near London and other smoky towns every insulator should be changed once every three months, or as often as required, but that system was never carried out. Those were days of great economy; and our directors thought the one greatest merit of all was to keep down the expenditure; they had constant returns before them, and thought if they saved a shilling or two in each man's wages, and dispensed with a little labour here and a little there, they were doing good service. I think in this instance they were mistaken, and in fact they gradually abandoned the system.

I had noticed that Mr. Walker employed white porcelain, and after testing it I came to the conclusion that it was a very promising material for our purpose. I endeavoured to get some made in England, and you can scarcely conceive the difficulty there was in obtaining them. Their cost was at first from 4*s.* to 4*s.* 6*d.* each, and even at this price out of numberless samples the majority turned out to be mere china clay. I accordingly visited every pottery district in England, and at last at Coalport and at Worcester I found porcelain makers able to manufacture them of good

quality, and by degrees the price was brought within limits that we could afford to pay. From that time these insulators came more and more generally into use both in England and in foreign countries, and wherever I go in the most out-of-the-way parts of the world I am often gratified by seeing the double-bell insulators in use. Mr. Varley subsequently effected the improvement of making the insulator in two pieces, each of which could be tested separately, and this is the form now in general use.

The next original step in advance is, I think, the one taken by Messrs. Johnson and Phillips. It is too early to do more than form an opinion, but I think there is something promising in it, and the idea is new. I have myself tried the experiment of oiling the inside of the insulator, and with some temporary benefit, but the oil became sticky, and caused the dirt and soot to adhere so firmly that it could not be cleaned.

I have no suggestion to make at the present time that would be of practical use, except to call attention to the merits of the enamelled iron stems which I used in my first insulators. We found the enamel was at that time not put on sufficiently well to ensure perfect insulation, and in consequence its use was abandoned both in England and on the Continent. But it is worthy of consideration whether it would not be possible now to obtain a more perfect enamel coating if attention were given to the subject, and whether an iron rod, coated with thick enamel or enamel and porcelain, in two coatings of considerable length, would not make a very excellent insulator. It would offer but a small surface for the current to pass over and one that would get cleaned by being constantly washed by the rain. It would require some kind of solid head to support the wire, in order that it might not be too easily destroyed by lightning.

In conclusion I will only remark that it would be a good thing if some of these old forms of insulators could be preserved. To that matter our friend Mr. Walker would probably be able to help us, as could also other gentlemen present connected with the different telegraph systems of the country. A collection of the early insutators would form a most interesting record of what the telegraph has been in the past.

The CHAIRMAN: On the last occasion of our Meeting we had not the advantage of Mr. Gavey's presence, and if any Member desires to ask any questions or to have any further information I am sure Mr. Gavey will be happy to respond at once.

Mr. WALKER: I have by me a series of insulators, commencing with the quill, which I will call No. 1, and a piece of felt, which is No. 2—I am not sure which is No. 1—and I have a series of some of the old insulators which I shall be pleased to place in the museum of the Society if they are deemed of sufficient value for your acceptance.

The CHAIRMAN: I may mention that at the Loan Exhibition at South Kensington there is a pole which has upon it every known form of insulator that could be obtained. The quill is not there—the felt is—and if Mr. Walker will leave us the quill in his will we shall be pleased to attach it to this specimen pole at South Kensington. The suggestion of Mr. Latimer Clark is worth the consideration of the meeting, and I am sure we should be glad to start a specimen pole on our own account. There were some remarks at the last meeting left unfinished by Messrs. Johnson and Phillips, and perhaps, as several specimens of insulators have been brought here by those gentlemen, one or both of them may have a few more words to say on the subject.

Mr. PHILLIPS: We have brought some insulators here to-night, and their dirty condition will give some idea of the severe conditions under which they have been tested. On the last occasion I referred to twenty insulators, which have been up for fourteen months, and have never fallen below an absolute resistance of between 1,000 and 2,000 megohms. On the top arm on the pole before you there is one of these insulators; below there are four others which were fixed immediately over a small iron smoke-stack ten months ago, the right hand one only being charged with oil. To-day the weather was very dry, and I thought the best way to try them was to get a jet of steam underneath the insulators, and having maintained this for about half-an-hour I could not get the slightest perceptible loss on the one which had been charged with oil, while the others were very bad indeed. There was a small wooden roof beneath these insulators which on Monday last caught fire, and the wooden

arm was charred. I had for a long time quite made up my mind that the oil must be dried up. We did not attach much importance to this trial since it seemed so outrageously severe. I was therefore both astonished and delighted at my yesterday's result, since it has an important bearing upon the time the oil will last in hot countries, and it also shows how independent we are of the porcelain becoming dirty. On examination the oil was found to be quite fluid and clear.

I am glad to learn that Mr. Gavey is here to-night, and I would like to call his attention to the values he obtained on our No. 5 insulator. At the last meeting I pointed out that on November the 9th, in fine weather, 664·16 megohms were obtained, that on the 12th it fell to 221·428 in slight rain, and on the 13th, weather damp, it was as low as 27·125, while on the 14th, in thick fog, it rose to 7,350 megohms. Again on the 17th, during fog, it rose to 8252·941 megohms, having been down to 2,480 megohms in the interval. Now it is certainly curious that values obtained during a fog should be higher than those obtained in fine weather, and I shall be glad to hear any explanation Mr. Gavey may have to give us on this point. With reference to that most remarkable insulator, No. 6 in the table, I see in one instance an absolute resistance of 132440. megohms was obtained after the insulators had been up fifteen days, during most of which it had rained or been foggy.

Multiplying this marvellous result by ten to obtain the insulation of one insulator, we get 1·32 million megohms, a value which I imagine must approach very nearly to the resistance of the porcelain itself.

I was very interested with what Mr. Latimer Clark said in reference to his having previously seen an insulator with its lower edge turned up inside in the same way as our No. 5 insulator,* a section of which is shown in the drawing, and I can quite confirm what he said as to its being a good form, quite apart from the oil, and as compared with ordinary insulators, but they nevertheless soon give out. I put up twenty of this form without oil some time ago, and for several weeks they gave splendid results, but now they are little better than the ordinary ones. The drawing marked No. 17 † is the other form which we sent to Mr. Gavey, an iron sheathed

* Fig. 21 in illustrations. † Fig. 10 in illustrations.

insulator, and the right hand drawing No. 20 is a smaller and cheaper form of No. 5.* No. 5 has rather a large oil chamber, but there is no doubt that No. 20 would last a very long time without requiring renewal of the oil.

Mr. W. C. JOHNSON then explained the specimens of the No. 5* pattern of the fluid insulators which had been exposed for ten months, on a level with and about fifteen inches from the top of an iron funnel. This was the chimney of a furnace which had been in constant action during the time stated.

He said all the specimens are now covered with carbon about one-sixteenth of an inch thick, and are just as they were when removed from the funnel this morning.

Of these two on the left-hand side of the arm, one still has oil left in the annular space, and the other is the one which was not charged with oil. The latter is quite loose on its pin—the heat from the furnace was so intense that it has melted away all the sulphur cement which secured it.

In the former, the oil still remains, and, electrically, the insulator is as perfect as when fixed at first; and I must mention that they were found to be in this condition before the accidental destruction of the building beneath by fire.

The next ones on the arm are an ordinary form of Andrews' insulator, put up to test comparatively with the others, and a No. 5 pattern fluid insulator. The latter was not charged with oil; both of these gave the low result when tested mentioned by Mr. Phillips.

The CHAIRMAN: What is the condition of the oil in the one in which the cement is destroyed?

Mr. W. C. JOHNSON: The cup had not been charged with oil, but it has a similar position on the arm to that one in which the oil remains. Flame was frequently emitted from the chimney-top, and I should have expected to have found the porcelain cracked under these conditions.

On another part of the pole is shown one of those which was fixed under ordinary conditions in January, 1877, and removed this morning; also some of the iron-clad forms. In these the oil-cup is capable of sliding down the pin, for the purpose of examining the condition of the oil or for cleaning. The cup is retained

* Fig. 21.

in working position by means of a binder of thick lead wire. A ring of india-rubber was at first employed, but was found to deteriorate rapidly when exposed to the atmosphere. But an objection has been raised to the employment of the lead wire in India, as it is probable that it might be stolen by the natives to cut up for shot. But that cannot be regarded as an objection that would hold good in England.

No. 13 is one of the larger forms of fluid insulators. The large iron hood has a large female thread cast in it which screws on the porcelain sheath and well protects it. It is similar in size and appearance to the brown-ware Varley form so extensively adopted in the postal telegraph lines.

Mr. Greener : I am very sorry I was not able to hear the paper read. This is one of the most interesting subjects, so far as my own feelings go, viz., the insulation of telegraph wires, and that I suppose is because I have been connected with telegraphs for so many years. I started very early in telegraph work—in Mr. Fothergill Cooke's days—and, as far as insulators go, Mr. Latimer Clark said I had something to do with the first telegraph on the Great Western line. That was not so. My career began first on the Blackwall line, and we did not have much overland line there, only a little bit between Poplar and Blackwall of about 300 yards, and about sixteen wires. In those early days there was a bit of a ring for the wires to rest upon, fixed by staples. The next thing was a little cone. The next thing I remember in the shape of an insulator was Mr. Walker's double cone, which he has stuck to throughout, and therefore I suppose he must find them answer; and then after that there were certain alterations. Mr. Hatcher had one or two when he was engineer of the old Telegraph Company. Then Mr. Edwin Clark made his earthenware insulator with a zinc cover, and then came the solid earthenware.

The next great improvement was, I believe, Mr. Latimer Clark's invert. That was the next great step as regards insulators, and with that insulator I have had some experience on long lines, that being a porcelain insulator with double cup. I had to do with a line of 2,000 miles, from Constantinople to Bagdad, consisting of two wires, one being insulated with Mr. Latimer Clark's invert,

and the other with Siemens' insulator with a single cup. The insulation on the Clark's porcelain insulator was always very very much higher than the other, but it was seldom we could get any distance upon it on account of the weakness of the insulator; it was not strong enough, and got broken. The single cup line was much lower in insulation in bad weather, but we could always manage to get through comfortably, showing that insulators want mechanical strength as well as electric qualities. This was in 1854, when Mr. Latimer Clark brought out that invert, and insulators of that description have been made from that time to the present. I have the honour to be inspecting engineer of telegraph stores for India. We have had many forms in India of double-cup insulators, but all have been very much the same thing as Mr. Latimer Clark's invert. We have had different shapes, but they have been no great improvement.

I think the time is come when, if I was an inventor, I should look about me to get a good insulator. I hope some of our young men will do so. I think really this insulator of Johnson and Phillips' is something in the right direction. I don't know whether it is *the* thing, but it is something out of the track trodden in for these number of years, and when it was first brought to my notice I said I did not believe in it—simply, I suppose, because I had been used to the old double-cup for so many years that I had no thought of anything else. I was invited to their works to see these insulators tested. I went there with Mr. Schwendler about last May, and we tested them after they had been up only about three months, with oil in them. It was very dry weather at the time, and we sent up a man to pour water upon them. There were about 20 of these double-cup insulators put up. These insulators were not affected in the least, whilst others went down to nothing. I felt then there must be something in these insulators, but I did not see how they could be used practically with the oil. I thought the oil would get out if it was upset, but it did not. I suggested to them that they should try experiments with regard to the cooperation of the oil, and they did so; and after some months I went again and I found the insulators in the same condition, and

tested as before, whilst the other insulators were, as before, low. I had one taken down, and I tried to get the oil out, but could not. It stuck to the sides of the cup, and when turned back it went into the trough again. I have taken several tests of these insulators to see how they went on, and for some time past I have felt I should like to come here and say something about them. Yesterday I got Mr. Graham, my assistant, to go to Charlton to test these insulators. The last time I did so the 20 insulators gave a resistance of 14,000 megohms. Yesterday was a fine day, and Mr. Graham found it was no use testing them, as the one was almost as good as the other. Mr. Graham went again this morning early, when there was a little fog and snow. The resistance of the 20 insulators of No. 5 form was then 2,756·8 megohms—absolute resistance of 20 insulators, which is very fair I think, giving a resistance of 55,137 megohms per insulator. There were four ordinary insulators tested at the same time, and the absolute resistance per insulator was 19·483 megohms, being 19·483 against 55,137 megohms, and that, after being up for 14 months. I believe the insulators had not been touched in any way. I went down myself again and saw these black gentlemen. I thought if there was any insulation about these fellows we must have greatly improved, and I had one that had got oil in it tested, also one without oil, and there was no deflection to be had upon them, whilst the ordinary insulators gave dead earth. After Mr. Graham came away, Mr. Phillips put on a jet of steam, and the one with oil in it remained the same, and the one without oil was bad, showing, I think, that the oil is doing its duty very well. I may say I have great faith in this insulator, and am glad to say that Major Champain has ordered 100 miles of them to be put up along a sea-coast line in India, which will be a good practical test for them, because if they will answer on the Mekran coast they will answer anywhere. At the present time it is necessary on that coast to have men constantly cleaning the insulators.

As to the different kinds of insulators and the porcelain they are made of I have had the testing of a large number, and there is a great difference in the insulating properties of porcelain. Of course, my experience in that respect is not worth much, because

my tests have been made when the porcelain was quite clean and dried with heat to do away with surface leakage, and I found that the English ware will give perhaps two millions of megohms to three millions; but the highest results I have obtained are really from Schomburg's insulators, and they gave, when clean and dry, from 10 to 12 million megohms. In a country like this I believe one would be as good as another in a fortnight.

There is one thing on reading the tables given in the paper, which is most surprising to me, and I would like to know what porcelain this insulator is made of, being so high, compared with all others, —that is the first on the list, No. 6. I have seen that form and examined it, and I have it in my mind with other insulators, and there must be something very peculiar about it. It must be excellent porcelain for the insulation to be so high. May I ask how long these insulators have been up?

Mr. Gavey: They were put up for the special occasion and were tested at once. Most of the other insulators have been erected and tested at various times previously. These, No. 6, I had just erected, and they had not been tested previously. The day after they were erected the tests commenced. All the others had been up and simply washed and cleansed before the testing was commenced.

Mr. Greener: Then these were new insulators, and had not been up previously?

Mr. Gavey: Yes. The time varied from three to twelve months. No. 6 were absolutely new insulators.

Mr. Greener: I quite understand that now. I would say, I do hope we shall now go on and see if we cannot improve our insulation. I am glad to see Mr. Graves here. I think it would be a good thing to test insulators for a length of half a dozen or a dozen miles. If we could get Mr. Graves, when the Society think they have a good insulator, to put up half a dozen miles on a Post Office line, that would be a good practical test. I do believe in practice. Theory is a very good thing, but I believe more in the other.

The Chairman: I may state that Mr. Graves has already erected experimental lines of insulators to a much greater extent than that. He has put up lines insulated with eight different insulators selected

from those which had given the best results in testing, including that of Johnson and Phillips, the others being selected from a great number experimented upon by Mr. Gavey at Bristol. These eight wires extend from Wesbourne Place to Uxbridge, 22 miles, and it has been a great misfortune that this month of March and February have been so dry that it has prevented the possibility of testing these wires under adverse circumstances and bringing the results before the Society, but no doubt we shall eventually be able to show excellent results from the testing of these experimental lines.

Mr. GRAVES: When rival inventors and rival professors submit their inventions and ideas to the Post Office it becomes my duty to consider them, and therefore I am somewhat chary in expressing in public the opinions I have formed in private. Last year various inventions in the matter of insulators were brought under our notice, and after discussion with Mr. Preece and others a determination was come to to make experiments with various insulators upon a selected section of line from London to Uxbridge *viâ* the canal, partly on account of the facilities of access and partly as presenting favourable circumstances for testing the qualities of the various insulators. Starting with some of the old forms of insulators, we went on up to the latest introduction of Johnson and Phillips, whilst at the same time the opportunity was afforded of comparing the merits of porcelain with those of brown earthenware. The arrangements for the experiments are complete, but, as Mr. Preece said, we have, as yet, obtained no results owing to the dryness of the atmosphere. I would mention one thing. We have heard in connection with the comparison of different insulators that it is unfair to that class which gives the highest resistance to weight them with shackles or terminal poles, and we are advised that we ought to take out of circuit portions of underground line that may intervene, and take the true value of the insulation, uninfluenced by any such conditions. Now that may be very well in theory, but it is impossible in practice to carry out a long line without intervening lengths of underground line, and I say at once that the insulator which gives the highest results under the circumstances existing, would

be the one that should carry the preference, because it is not always that the highest electrical qualities give the best practical results combined with the greatest economy of construction.

Mr. Greener: With regard to shackles, underground wires, and terminal posts, I think you may have the finest insulator that can be produced in point of resistance, but there is always something that will break down your lines as long as they are lines. Mr. Preece will bear me out that there are always bad weak places in a line, and except those are kept good it is no use to have good insulators; and I do not see that it is a fair test for insulators unless those faults are kept out of the circuit, because, be as careful as you may, if there are faults in the line those faults may unjustly be attributed to the insulators. Many years ago Mr. Preece had some lines under his charge, and under his instructions I had to go from station to station and pick out the bad places. We could not work through more than ten or twenty miles. They were altered, and then all was right. I think, if it is possible to test insulators clear of those things, it would be a better test.

Mr. Graves: You are quite right in one respect; if you are testing the electrical qualities of an insulator, the more you are free from disturbing elements the better; but if you want to determine the merits of an insulator for practical purposes the existing conditions attaching to all telegraph lines must be borne in mind. While it is desirable to give attention to the subject of insulation in the direction in which we are going, it is equally desirable, in fact more important, that attention should be given to the subject of terminals and the connection of underground wires with overground. We are more let down by that than anything else, and it requires the greatest care. We find deterioration comes on gradually, and it cannot always be detected in time to prevent the evil before it occurs. With respect to insulation, it is necessary to give attention to the terminals from one end of the circuit to the other.

Mr. Johnson: As to the terminals of the line and the shackles, they were points that Mr. Greener brought to our attention at an early stage of our labours in fluid insulation: and I think Mr. Greener will admit that they were well met by Mr. Phillips as to the connection of land lines and terminals. The terminals can be

easily effected in the same way with fluid insulators, and a very elaborate insulator for connecting underground wires with aerial lines was produced by Mr. Phillips, which I believe will be put into practice in the test to be made on the Uxbridge Canal.

Mr. R. K. Gray : I think the question of Johnson and Phillips's insulator as against the others may be very simply considered. The whole thing lies in considering whether the insulation of the surface of oil is greater than the insulation of the surface of the porcelain of the insulator. In making the test it appears to me quite sufficient to test the insulating properties of the oil, as compared with the porcelain, quite separately from the testing of any other insulator. Looking at the results of tests of other insulators as against those of Johnson and Phillips, it appears that no person has determined the value of the porcelain surface as compared with the surface of the oil. It would be easy to arrive at a conclusion whether the oil is better than the porcelain.

Mr. Willoughby Smith : I think something like eighteen months ago, when Mr. Phillips introduced this insulator to me, I suggested that if he would like to place two or three of them in my grounds he could do so, and they might remain there. It is just to say I examined them yesterday. He had put up two kinds of insulators; one was what is called the invert. I suppose the oil had got down the side, and it very much resembled what is called a "Catch-'em-alive O," being thickly encrusted with different kinds of insects. In the form we see here to-night the insulator is as clean as possible, and there are no signs of oil having left the cup. I should say if these were tested they would be equal to what they were when they were first filled fifteen or sixteen months ago. I am not much interested in aerial insulation, my hobby being, as you know, subterranean work, which you will have to come to in the end. I will take the first opportunity to test the insulators in my grounds, but I have not done so yet. They are hung amongst the trees, where they have been for the last sixteen months. When I first took the place there was the voice of the nightingale and the smell of the rose; but now a gasworks has been placed there, and I think you could hardly have a worse place to fix the insulators in; but I am pleased

to find that the atmosphere of Charlton is more impure than where I reside.

Mr. Johnson wished to mention that the form of insulator to which Mr. Willoughby Smith referred as offering a tempting bait for flies had been experimented upon by Mr. Graves amongst the first, but that was sent to Birmingham, and that form had since been abandoned, and, therefore, need not be taken into consideration in the discussion.

Mr. Jobson (responding to the Chairman's invitation) said: I have had no practical experience of the testing of insulators. We are manufacturers only, and not scientists, for which I am thankful, for we must all agree that tests of this sort must be in a great degree fallacious, as practice alone can bring out the real merits of a thing.

Mr. Gavey: I do not know that I have occasion to reply to many of the observations made by members on the paper which is before the Society, but there are one or two points to which I would refer. In the first place, Mr. Phillips raised a question as to certain discrepancies in the tests recorded of the insulators numbered 21. These I can explain. I must confess that I was at first disappointed on comparing the results I obtained with those published by him, and, as I had some reason to think that damp had invaded the interior of these insulators before they were filled with oil, I had them taken down on the third day, exposed to a temperature sufficient to drive away every particle of damp, and then re-erected; after which the resistance rose in a remarkable manner. I think that is a simple explanation of the results. Subsequently this insulator dropped down again. The explanation of that I cannot give, unless it be that the oil was somewhat defective. I used rosin oil, but it was not subjected to any special test to ascertain its purity or electrical resistance, and it is possible, inasmuch as my results do not bear out those of Mr. Phillips, the fault lay with the oil. With reference to the question raised by Mr. Gray as to the relative resistance of the glaze and of the oil, I may observe, that, in this special application of Johnson and Phillips's, it is not so much a question of the specific electrical resistance of each substance, as of its affinity for moisture (assuming, of course, that

both are good insulators) that has to be considered. Of course, if you take a piece of good porcelain, and by suitable precautions prevent all deposit of moisture on the surface of the glaze, you can scarcely obtain any evidences of direct conduction by means of an ordinary Thompson's reflecting galvanometer; but, if you take any series of insulators and expose them to the atmosphere of this country on the driest day, you can always get a certain degree of deflection, which shows that there is a film of conducting moisture existing along the surface. I have tried it on a bright sunny day, and have obtained a definite reading on ten insulators. On placing these before a fire for an hour or two, so as to dissipate the surface moisture, this deflection has wholly disappeared. Oil having no affinity for moisture, we may assume that there would be practically no deposit of moisture on its surface, and it would therefore be free itself from this evil of surface conduction, and would likewise protect that portion of the porcelain insulator to which it was applied.

On the question of the extraordinary resistance given by the No. 6 insulator, I confess no one could be more surprised than I was when I obtained those results. I owe an apology for placing before the Society a paper in which I did not give some explanation of them, but the paper itself came on earlier than I anticipated, and further, just as I had concluded the series of tests recorded, certain departmental changes involved the removal of myself and the whole of my staff and apparatus from Bristol to Cardiff. Two months elapsed before I was prepared to resume my experiments in the latter place, and finally, as Mr. Preece has mentioned, we have had such extraordinary dry weather for the last six weeks, that little of any value could be achieved. Still I have had some results, which, perhaps, may tend in a certain measure to explain those I obtained before.

I may say my first impression was that the india-rubber ring on the insulator, which is interposed between the cast-iron bolt and the female screw, took up the role of the oil in Johnson and Phillips's insulator. That is, it was far away from the direct action of the elements, and having little affinity for the moisture of the atmosphere it remained comparatively dry under all circumstances, either of rain,

fog, or other atmospheric changes. Naturally the first thing I did was to test a series of insulators with the india-rubber ring, and another series without; not, by-the-bye, the insulators which had given these results in Bristol, but another set of the same description. I may say at once the former results were neither obtained nor reached in any degree, and I will give a reading of one test made on the 28th of February, with a long insulator, with a washer, similar to No. 6. That gave a resistance of 22 megohms, while the insulator without a washer gave 20·9. A series of short insulators of the form hitherto used by the Post Office, fitted with screws and washers, gave 5 megohms. A short insulator fitted with screws, but without the india-rubber washer, gave 25 megohms. The ordinary porcelain insulator of the same description, but with bolt fitted on in the old manner, gave 2·2 megohms. Mr. Varley's insulator gave 3·4 megohms. Later on I had the insulators that had given such extraordinary results at Bristol re-erected in Cardiff, on the pole containing those now referred to. Unfortunately, since they have been up we have had no rain, or rather none in the day-time, when an observer was on the spot for testing; but on the 21st of March, it having rained heavily in the night, a series of tests was taken in the morning, and I may say at once the insulator that gave such extraordinary results before now dropped to the same level as the last series of insulators, which I erected to endeavour to trace the cause of the former results. In other words, the long insulator, with washers, gave 1,250 megohms; without washers 1,041. The short insulator, with washer, gave also 1,041; without washer 1,550. The ordinary porcelain insulator, of the Post Office form, gave 833; Mr. Varley's 833; and No. 6 gave 1,550.

Mr. Graves: When you speak of results, were these tests of a single insulator or an average of several insulators?

Mr. Gavey: Seven insulators were put up together and tested together. To some extent these further results would appear to show that the extraordinary resistance first recorded was mainly due to the influence of the india-rubber ring, and the fact that the original resistances were not reached in any later instance is readily explained by considering that in the course of three or four months

the india-rubber, by exposure to the atmosphere, became deteriorated. The insulators No. 6, giving the original figures, were erected as soon as they reached me from the factory. When re-erected they had been exposed for some months to the atmosphere—a sufficient length of time for the india-rubber to fail electrically. Similarly with the other insulators of like pattern, tested in Cardiff, the bolts, with their india-rubber washers, had for some months been exposed to the atmosphere in the store depôt, so that they failed to have any effect on the total resistance of the insulators. Allowing, however, for this temporary action of the india-rubber washers, the fact remains that certain of the screwed form of insulators gave a much higher average result than those fitted with bolts in the ordinary manner. On closely examining some of the No. 6 screwed insulators, I found a portion of the inner cup, for a length of about one inch downwards from the point on which the washer presses, free from glaze. Now we all know the value of a good glaze on an insulator, but we likewise know its disad vantages. We are aware what a very strong affinity atmospheric moisture has for glass. If you put a glass vessel of any description in an atmosphere which contains any moisture, you will get moisture condensed on the surface, and if you put a highly-glazed insulator in any atmosphere in this country you will always get a visible reading on a delicate galvanometer, showing that there is a deposit of moisture on the surface. Now, glaze on a porcelain insulator is simply glass, and with a highly-glazed porcelain insulator you have, as far as condensed moisture is concerned, very much the same results as you would have with a glass insulator; and I need not point out how difficult it is to keep glass from condensing moisture on its surface.

It strikes me it is very possible that part of the high resistance recorded in this insulator is due to the unglazed portion of the inside of the insulator. In saying this, I must not be thought to advocate the use of unglazed porcelain for insulators. We all know the utility of the glaze on certain portions; but I think we should obtain better, and more and more uniform, results, by maintaining that portion of the insulator which is removed from the direct action of the elements unglazed, or by forming the interior of

the insulator of some substance which has so little affinity for moisture that unless moisture in the shape of rain has direct access to that portion of the insulator it will maintain to a great extent an unchanged surface. Messrs. Johnson and Phillips have endeavoured to maintain that unchanged surface, and, as I have said, great credit is due to them for having introduced an entirely new form of insulator, and made a step very far in advance of all others previously introduced. Possibly, practical experience may raise an objection to that form which we do not see now. That is a question for the future, but there may be means of obtaining the same results in a more economical manner, or perhaps in a manner which may better meet practical requirements; but, although they have made a great step in advance, I do not think it should prevent others from endeavouring to arrive at the same results in another manner.

The CHAIRMAN: In according our thanks to Mr. Gavey for the paper he has brought before us, I am quite sure I express the sentiments of all present in saying we appreciate very highly his efforts, not only for the valuable paper itself but also for the extremely valuable discussion it has elicited. Mr. Gavey did not go much into the history of the matter, but he has been the means of bringing before the Society, in an extremely interesting manner, some of those most interesting historical facts associated with the insulation of telegraph lines. We have heard one of the principal inventors in that field to-night, Mr. Latimer Clark. We have also heard what Mr. Walker has said, and I am bound to say I can confirm, to a great extent, what Mr. Walker has said with regard to his cone insulator. Lines with Mr. Walker's cones have come under my superintendence. I have had to test those lines, and have been astonished to find that wires insulated with Mr. Walker's cones have tested as well as those carried by the higher classes of insulators. I think we must all agree, from the remarks we have heard, that the true standard of insulation is not so much the resistance of the insulators themselves as the resistance of those numerous points of leakage—faults in fact—which occur at the mouth of every tunnel, at every bridge, and at every connection of underground with aerial lines. In a country like this it is impossible to carry a wire twenty miles without terminals at two

or three points. It is to these weak points we should now look. Mr. Greener called attention to the fact that some years ago we had under our charge a very bad line indeed. As he has said, we could scarcely work through twenty miles, and the first thing we did was to attack those weak points. We cut out every terminal, and the result was that a better working line does not now exist. The insulators were the same, but the weak points were removed.

There is one point connected with insulation which has not been much dwelt upon—that is, the influence which climate has upon it. We have spoken almost exclusively of our experiences in England. Mr. Greener alluded to his experience abroad, but the paper itself, which will be circulated all over the world, will be read by anxious minds, who will compare the condition of insulation in their countries with the condition of insulation in England, and they will look at the fact that their insulation far exceeds that which we experience in England. I was struck with this during my tour in America. There I found the lines tested most beautifully, whilst the insulators, without exception, were the most execrable I ever saw. I am certain if any line out of London were insulated with American insulators you could not possibly work: yet they are able to work through thousands of miles. I had a letter from Mr. Hamilton, a leading American electrician, who was in this country a little while ago, stating that nothing struck him so much in England as our wretched insulation. He found that the best wires out of London tested worse than the worse lines in America—forgetting, it may be, the fact that the climate of the two countries is very different. In fact, the climate of America is so dry that you do not want insulators at all. In this country there are certain periods of the year, especially in November, when very strong meteorological phenomena occur. In the month of November there are aqueous clouds which envelope all the country, charged with moisture, which renders the most perfect insulation quite as bad as the worst insulation ever constructed. Those great aqueous clouds are not experienced in other countries. In America the wind blows from the sea to the land—but it blows from a cold climate to a hot one. In England the winds sweep over the ocean from the warm Gulf-stream to our own shores, which are much

colder, and they come over the land charged with moisture, which they deposit, and cover the insulators of the lines with films of moisture; and that is the reason why our lines work so badly, and hence it is impossible for our friends on the other side of the Atlantic to draw any comparison between the insulation in their own country and the insulation in England.

There is another point which has been touched upon—that is the necessity for washing insulators. It is all very well to design perfect insulators, whether with the adjunct of oil or porcelain only; but we must pay regard to the practical difficulties involved in keeping a line perfect for more than three months. At the same time, we must remember, in carrying out a long line like that from London to Glasgow, you cannot send men out to wash the insulators every month or two, as you can do when you make experiments in your own laboratory. We cannot clean the insulators every day—but we ought to be in a position to clean them in dirty places periodically; therefore, in the practical construction of insulators we must have regard to facilities for cleaning and keeping them in a proper state of maintenance. Hence it is, that the *electrical* qualities of insulators are not the only points to be considered. Much has been said to-night about the electrical qualities of insulators. Deductions have been drawn from tests made on a few insulators placed conveniently, so as to bring the wire into the testing-room; but the actual test of a line is the test which Mr. Greener shadows forth as being about to be adopted by Major Champain in a foreign country and by Mr. Graves along the Uxbridge Canal; but it is impossible for practical electricians to come to a definite conclusion as to the merits of insulators till they see how those difficulties are surmounted which are presented by tunnels, bridges, stations, and even streets, besides the other obstacles which practical telegraphists meet with. The tests at present made are, as Mr. Gavey points out, very anomalous. Sometimes the most unsatisfactory insulator gives the best results, sometimes the finest insulator gives the worst results. We must bear in mind the fact that on a line of 100 miles, if there is 1 per cent. of bad insulators, it lets down the strength of the current 20 per cent. You must regard every point of terminals, and you can understand how, with these nu-

merous terminals, the strength of your current is liable to be run down. Hence it is that Messrs. Phillips and Johnson have devoted their principal attention to eliminating apparent defects, and have endeavoured to apply their oil system, which is a new step in advance, to perfect that which in practice is found to be an extremely defective form. We may say as much as we like, but there is only one means of testing and deciding upon the insulator of the future—that is the actual test which we are now going to carry out. I am sure you will heartily award your thanks to Mr. Gavey for the paper he has brought before us.

A cordial vote of thanks to Mr. Gavey having been passed, the Meeting adjourned.

The Sixty-seventh Ordinary General Meeting was held on Wednesday, the 10th April, 1878; Dr. C. W. SIEMENS, F.R.S. President, in the Chair.

The Secretary read the following paper.

ON THE LAW OF INTERNATIONAL TELEGRAPH TRAFFIC.

By C. L. MADSEN,

Local Hon. Secretary for Denmark.

When I had the honour to be invited to deliver a paper "On the Law of International Telegraph Traffic," the subject of some recently published treatises,* I intended to be present at the reading, and at the possibly ensuing discussion of the paper. I regret that I have not been able to do so, and for this reason, and in order to prevent misunderstandings as much as possible, I propose to give a more extended *résumé* of the treatises, and to deal more fully with the problem and the results of the researches than otherwise would have been requisite. I have, however, considered it my duty in this paper to treat the law of international telegraph traffic chiefly from a telegraphic point of view, and not to enter into considerations of a geographical, statistical, or purely commercial nature.

In thus having the honour to bring a novel theme before our Society, whose members during a quarter of a century have been actively engaged in all parts of the globe in establishing, organising, working, and perfecting that grand system of nerves which now carries by far the greater part of the whole international tele-

* Recherches sur la loi du mouvement télégraphique international. Published by E. Dentu, Paris, 1877, and—Nye Undersögelser om Loven for den internationale Trafik. Foredrag i det kgl. danske geographiske Selskab. Kjóbenhavn, 1877.

These researches were first made the subject of a treatise, read before the Society of Political Economy at Copenhagen on the 10th of March, 1876, and at the request of the said Society the treatise was published in extenso in Danish and French.

graphic activity, I need scarcely add, that in no other society are so many of the members, from their accurate knowledge of the amount, nature, and cause of traffic, not only in the position to judge of the importance of the theory, but also to contribute materially to the further development of that law, which forms the subject of this paper, and the basis for extended investigations.

The subject naturally divides itself into two parts: establishment of the law and the application of the law.

The fundamental idea, which finally led to the establishment of the law, rests upon the simple and well-known fact, that by far the greater part of all international messages concern under normal circumstances, directly or indirectly, the commercial traffic only, a fact that has been pointed out in an able statistical treatise by Sir James Anderson. It is as far back as 1859 that I first noticed the same fact, and it was when I made the parallel movement in the totality of the international traffic of the Scandinavian countries the subject of statistical researches that I conceived the idea of the existence of a mathematical relation between the international, commercial, and telegraphic traffic.

The absence of, or the extreme difficulties in obtaining, sufficiently authentic and detailed accounts of international traffic, embracing at the same time the telegraph, trade, navigation, &c., have rendered it a necessity to found all these researches on the official statistics of Denmark, Sweden, and Norway only, the statistical returns of these countries giving all details of the commercial and telegraphic relations of these countries to the rest of the civilised world.

In the attempt to represent the international telegraphic traffic as a mathematically determined function of the commercial traffic, I found the only guide and touchstone for the formation of the equations in the relation between England on one side and Sweden and Norway on the other side, the geographical position of the two latter countries in respect to England being very nearly alike, the traffic considerable, and at the same time totally differently composed—in the case of the English-Swedish traffic the trade forming the chief part, and in the English-Norwegian traffic the navigation. After numerous fruitless efforts I arrived at an empirical law, which,

having been fully and minutely discussed and applied to 77 classifications or groups of *differently* composed traffic between *differently* situated countries, discloses the most perfect harmony in the movement of the values of the factors of which the law is composed, as compared with the traffic itself, and the general development of international communications.

In the empirical law—

$$T = C \{ \sqrt{V . N} + N_1 + N_2 \} \; . \; . \; . \; . \; . \; . \; . \; (1)$$

T denotes number of telegrams.

C — a variable co-efficient.

V — value of trade, expressed in pounds sterling.

N — tonnage of vessels engaged in the direct trade between the two countries L and L_1.

N_1 and N_2 denote the tonnage of vessels belonging to either of the countries L and L_1, and engaged in the carrying trade of L_1 and L.

Of these factors the term $\{ \sqrt{V . N} + N_1 + N_2 \}$, or *the commercial factor*, is the fundamental factor for the telegraphic traffic; the value of this factor expresses, under the name of "*commercial units*" = *U*, the total amount of commercial traffic howsoever this traffic may be composed.*

The variable co-efficient C is the more important factor in the law. It is the co-efficient which converts the commercial into telegraphic traffic; it is, therefore, the co-efficient which governs this *rapport*, and which *en bloc* comprises all elements together determining the amount of telegrams which a given amount of commercial traffic (units) produces. Provided, therefore, the commercial unit has been correctly defined, it is in the value of the co-efficients that we shall trace and find the law, that is to say, all the elements of whatever nature they may be, telegraphical or geographical, which influence the telegraphic traffic besides the commercial factor, the very source of all traffic.

It is clear enough that in the traffic between countries, telegra-

* It will be easily understood that the commercial factor, generalised, comprises the *value* and the *transport* of goods. The transport by means of railway has, however, for several reasons, not yet been taken into account, but it will scarcely offer any difficulty to express this transport in accordance with the tonnage of vessels.

phically and geographically *uniformly situated*, the co-efficients must be very nearly alike. Consistent with this theorem we find that, when classified according to their numerical value, the 77 co-efficients form certain classes or groups, comprising certain uniformly situated countries. This result could not have been obtained except on the sole condition that the commercial unit was correctly defined, and that it formed the true expression of the commercial traffic. As a proof of the accuracy with which the law works, even at this early stage, I may mention the correctness with which I have been able to calculate the effect of an (unknown) alteration in the principles for the formation of the official Norwegian statistics of navigation, and of which a full account is given in the treatise. I observe, further, the striking harmony between the co-efficients for the traffic between Denmark, Sweden, and Norway on one side, and America, Asia, and Australiaon the other side, the co-efficients being respectively 0·000785 – 0·000726 and 0·000759. Even the small irregularities in the co-efficients for Holland and Belgium serve as a proof of the law, because these irregularities are easily explained by the transit trade through Germany, whereas the telegraphic traffic has been carried on direct between the trading countries.

The co-efficients indicate the intensity of the telegraphic traffic, in other words, they determine the amount of telegrams which a given amount of commercial units, say 100,000 units, produces. In order to obtain a clear and distinct insight into the elements which exercise the more deciding influence on the value of the co-efficients, I reduced all the traffics to one and the same standard of commercial traffic, viz., to 100,000 commercial units.

I shall quote here the table constructed for this purpose, which illustrates the relation between the geographical situation of the countries and the corresponding *actual* commercial and telegraphic traffic.

100,000 *commercial units* produced in the year 1873 the following number of telegrams in the traffic between

and	Sweden	Norway
1st group—including for Sweden: Norway, Germany, Russia .	2,393	2,407
„ „ Norway: Sweden, Germany		

			Sweden	Norway
2nd group—	„	England, France, Holland, Belgium .	1,110	1,205
3rd group—	„	for Sweden : Spain, Portugal . .	830	791
	„	„ Norway : Russia, Spain, Portugal		
4th group—	„	America, Asia, Australia . . .	73	76

Owing to its central situation, *Denmark* occupies opposite *Sweden* as well as *Norway* an equal position of a higher order,. the co efficients showing a traffic of a still more intense character than in the first group. The continental states and the southern countries of Europe and at the Mediterranean would complete the interval in the intensity of the traffic observed between the third and fourth group, but these countries are necessarily excluded on account of the extreme smallness of the traffic.

On closely examining and investigating the number of telegrams resulting from this equalisation of the traffic between the different countries, and observing the very great regularity which disstinguishe the movement of the co-efficients within the same group of countries, and the most characteristic interval found between the co-efficients of the different groups, I arrived at the fundamental law for the co-efficients, that is to say, for the intensity of the international telegraphic traffic :

The co-efficients are in inverse proportion to the distance between the countries,

or,

The telegraphic traffic between two countries is in direct proportion to the amount of commercial traffic, expressed in commercial units, and in inverse proportion to the distance between the countries.

It is evident now that the variable, and as a rule increasing, co-efficients contain a *constant element*—the distance between the countries. Consequently, each co-efficient dissolves itself into two factors,

$$C = \frac{c}{d},$$

of which d is the distance and c the variable element, indicating, when determined for each year, the *accumulated progress* in the telegraphic and commercial organisation and in the general international intercourse.

At the beginning of the period when the international telegraph

commenced to work,* the status and condition of the means of international communication was equal to 1; now, when the progress, accumulated at the end of a period of n years, is uniformly divided over the whole period, which very nearly must be the case, the annual progress is equal to p. *The complete equation* for the telegraphic traffic at the time of n years after the introduction of the international telegraph assumes, therefore, the expression,

$$Tn = \frac{(100 + p)^n}{100^n\ d} \left\{ \sqrt{V.N} + N_1 + N_2 \right\} \quad . \quad . \quad (2)$$

If in this equation the progress p, or the time n, or both these factors, are $= 0$, then we obtain what I call *the normal equation* for the international telegraphic traffic,

$$T = \frac{1}{d} \left\{ \sqrt{V.N.} + N_1 + N_2 \right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In the above I have stated in a very condensed form the results which step by step are arrived at in the researches, and which in the treatise are given in full detail, supported and confirmed by the logical and mathematical consequences of the officially stated facts, which the different statistical tables accompanying the work contain. Those who take an interest in the problem before us, and who wish to follow up these researches, I must refer to the published treatises, but some of the more important observations may here briefly be quoted.

The average variation from year to year of the co-efficients is in many places of so limited extent as to fully illustrate the regularity which so highly characterises the movement in the international telegraph traffic The steadiness of this traffic must partly be ascribed to the circumstance that variations or movements up and down in the commercial traffic are to a certain extent counterbalanced or equalised by the (as a rule) more uniformly increasing value of the factor p. In cases where a larger difference in the trade and navigation is observed, and where at the same time the organisation of the international communications may be said to be of greater stability, a parallel movement between the number of telegrams and the number of commercial units is observed. This is

* In these researches the year 1855.

particularly the case in the movement of the Danish-Norwegian traffic; it demonstrates that even between such countries, where messages of domestic character to some extent might be expected to exist, the commercial messages, nevertheless, are absolutely predominating.

It is interesting to note that the average variation of the co-efficients is found to be about four per cent., that is to say, that the annual progress in the telegraphic traffic may be calculated to about four per cent., *irrespective of the annual progress in the commercial traffic.* The general advance in the telegraphic traffic is therefore depending on the increase of *two* factors, and if, for a number of years, the augmentation in the value of these factors, p (progress) and U (commercial units) is found to be equal, then the telegraph traffic for the next period may be calculated to advance under similar circumstances according to the equation—

$$T(1855 + n) = \frac{(100 + p)^{2n}}{100^{2n} . d} (U1855) \quad . \quad . \quad . \quad . \quad (4)$$

In this equation the period is reckoned to commence with the year 1855, the trade, navigation, &c., and the general condition of international intercourse for that year being taken as basis. It will further on be shown how the plan of dividing the development in periods may be applied to the periods of the triennial telegraph conferences.

The law of distance is the basis of the telegraph traffic-law as well as of all others. This important factor is therefore in the treatise made a subject of special and extended study and researches. When we consider that the average value of the commercial transactions increases with the distance, just as the average tonnage of the vessels employed in the trade, it naturally follows that the same number of telegrams in the trade between remoter countries will cover a larger amount of commercial units, or a greater value of goods and tonnage, than in the trade between nearer countries. It is therefore but natural that in this analysis of the telegraphic traffic the distance should disengage itself from all the other factors of which this traffic is composed. I confess, however, that I, like so many others, was taken by the popular but erroneous belief that the marvellous invention of the telegraph, and the rapidity with which

it has spread its net over the whole globe, had rendered mankind master of time and space. Not, therefore, expecting to meet in these researches the law of distance, I felt, nevertheless, bound to accept the same, like all the other consequences of the inflexible logic of facts and figures. The close examination of the law of distance* proves that this law holds good even under the most perfect organisation of the telegraph (*la conversation télégraphique*), and however the international communications and transactions may be organised. How is it otherwise to be explained, that the enormous commercial traffic with India, Australia, and other remote countries, produces but a comparatively small amount of telegraphic traffic? And how can it otherwise be explained that this traffic has not to any great extent been influenced by all the administrative efforts to increase the same? It is nevertheless true that "the telegraph makes the world small," and that in the popular sense the countries seem to approach each other. The explanation is as easy as it may prove to be interesting. From one of the published tables (Table VIII.) it will be seen that the factor *c*, *the accumulated progress*, has advanced from the value of *one* in 1855 to about *two* at the end of the year 1873. From reasons which presently will come forward, the exact value of *c* cannot yet be stated, but it is very probable that *c* may now be calculated to be of the value of between 2 and 3. It is evident now that the *increase* of the factor *c*, *sum of progress*, might as well be expressed in the adequate *decrease* of the factor *d*, *distance between the countries*, hence the result—the progress achieved in the international intercourse since 1855 is, as far as now can be stated, of the same effect as if the distance between the countries had been reduced to *one-half* or *one-third*. This same effect may be expressed thus: 100,000 commercial units produce now, when employed in the same trade and navigation, about two or three times the number of telegrams of what they did in the year 1855.

Thus the increasing value of progress converted into decreasing value of distance affords a medium for determining in figures that effect, which has rendered the telegraph the most popular of the

* Recherches sur la loi du Mouvement Télégraphique International, pages 39–47.

present means of communication: the ratio at which the countries apparently approach each other, and at which the globe becomes smaller.

According to the precise definition, the distance between two countries refers to the length of the telegraph line between the corresponding commercial centres of gravity, this length being measured in geographical miles. The more the route between these centres is shortened the smaller becomes the value of d, and consequently the greater the value of the co-efficient C. This relation between distance and the co-efficient explains the power which experience shows that "*direct telegraphic communications*" always have exercised on the development of the traffic, the more so, as such communications very often afford not only a new and shorter route, but also a special line for the transmission of a special traffic. The effect of such lines is therefore observable, as well in the factor c as in the factor d, and they work in the same accelerating sense in both these factors.

Before however the distance thus defined can be accurately determined, it will be necessary to fix the position of the commercial centres of gravity. This is an operation requiring most extensive and careful labour, because each such centre must be determined by means of the geographical position and the traffic of each place, which partakes in the commercial traffic with the country concerned. The first attempt to solve this problem has been fully described in the treatise "*New researches on the Law of International Traffic*,"* from which a few observations may be here noted. The determination of the position of twenty-seven commercial centres of gravity in Denmark and Sweden and in the surrounding countries in the North of Europe is based upon the calculation of 5,488 moments. The Danish commercial centre of gravity in England lies 4° 3′ west of the meridian of Paris, and 54° 8′ N. lat. (about 60 statute miles) north-west of Hull, the English centre in Denmark respectively 9° 38′ east of Paris and 55° 45′ lat., or about 25 statute miles north-west of Copenhagen. Now the straight line between these centres indicates, according to the defi-

* Read before the Royal Danish Geographical Society at the Meeting on the 29th of October, 1877.

nition of physical centres of gravity, the shortest route between the countries concerned. If, therefore, postal, telegraphic, or other international communications are arranged so as to follow as near as possible this ideal line of communication, the service may be said to be "rationally organised;" if not, there will be a more or less considerable saving in time in altering the route according to the position of the commercial centres of gravity. If, for instance, the postal service between England and Denmark were to pass *viâ* Hull and Esbjerg Harbour (on the western coast of Jutland), the gain in time would amount to more than 25 per cent. of the time now spent on the present route *viâ* Ostend. A new postal route between Denmark and Germany, founded on the same principles and fully organised, would yield a still more considerable saving in time. We cannot here deal further with this subject, although it is one of great commercial interest, but it will not escape the notice of the Members of this Society that the theory of the commercial centres of gravity, originally intended to be of service only for the exact determination of one of the factors in the traffic-law (the distance between the countries), evidently seems destined to acquire an important signification of its own.*

When the position of the commercial centres of gravity has been fixed, the means are given to determine the value of the factor d, and consequently also the exact value of the factors c and p (accumulated and annual progress.)

I have thus endeavoured in the above *résumé* to give an outline of the facts and reasonings and the method which have led to the present form of the law of the international telegraph traffic.

* The determination of *the commercial centres of gravity*, as calculated by means of the telegraph traffic, offers many points of interest. A few may here be mentioned. The Danish commercial centre of gravity in Sweden has been calculated from one month's traffic in 1875, and from the whole year's traffic in 1876. Notwithstanding this enormous difference in the basis for the calculations, the difference in the position of the centres amounted only to four minutes longitude, or to about one-half of a geographical mile. This remarkable result confirms a most important fact, *the stability of the international telegraph traffic*, a fact which however can be traced through all the researches as well as in the traffic itself. *The commercial line*, as determined by the foreign commercial centres of gravity, may be considered as a measure of the extent of the sphere in which the international affairs of a country move. The contraction or extension of this line will therefore form a subject of great interest for commercial and general observations.

This traffic is thus shown to be composed of seven factors, of which the definition and function are given, and at the same time a few of the many results, which in a more remarkable manner confirm the harmony that exists between the working of the law and the reality upon which the law is founded. Of the seven factors four include the commercial traffic and navigation, and one factor, the co-efficient, has in course of the researches been dissolved in the three elements, *progress*, *time*, *distance*, which, as far as at present can be seen, constitute the most noticeable factors on which the intensity of the traffic depends. Further theoretical investigations have drawn attention to a new factor "*the commercial organisation*," which has been fully discussed in the "new researches" mentioned above. The commercial organisation means the average value of the commercial transactions; the more the trade is carried on in detail the more the telegraph traffic flourishes, and the factor representing the commercial organisation must therefore necessarily be defined and taken into account, the more so when the factor *d* (distance) is regarded as a constant. The special theoretical researches prove that the commercial organisation in the period concerned has shown but a small and irregular variation, the factor cannot therefore be defined, because the statistical returns give no statement of the average value of the commercial transactions. The question is, however, one of great interest for the tariffs, and the matter will therefore again come forward under the head of the application of the law.

In conclusion, I may remark that the law of the international traffic thus will be found to have acquired a certain degree of completeness, and to be in all parts in full development, so that one day we may expect to be able to ascertain, define, and bring into account all the elements and factors which are now concealed in the enormous traffic, and in the chaos of figures and facts which accumulate from year to year.* Nothing will contribute more to solve this problem than the careful and extended study and analysis of the international telegraph traffic.

Some of the members may, perhaps, be of opinion that it is rather early to discuss the application of a law, when the law itself

* See preface to the treatise, "Recherches sur la loi," &c.

has not been put to any other tests than those which necessarily are confined to the sphere in which the law has its root. As the law, however, has been able to stand these tests, and as the investigations and researches prove that the space left for the action of new factors is not considerable, and in most cases already brought forward in the movement of the co-efficients, I think that consequences and conclusions which may be arrived at from a *correct* application of the law may be found as reliable, as interesting, and useful.

A question which immediately presents itself is the question ot the probable traffic on a proposed telegraph line, or on a line not fully developed. The equation (3) will answer that question, but it will require some local examinations before the value to be allowed the expression of $C = \frac{1}{d}$ can be settled. In an enterprise between remote and isolated countries, without any or with only unimportant means of telegraphic and other communications, and but a small commercial traffic, the co-efficient can scarcely be expected to exceed the value of $\frac{1}{d}$; in other cases, where the countries are well populated, and commercially brought up to a "European" standard, the co-efficient may be taken equal to that of a fully developed traffic.

When the question in regard to the amount of traffic is settled, we meet the question of the tariff to be employed, upon which it chiefly depends whether an enterprise will pay or not. The proper tariff may now be determined either directly or indirectly; in the first place, simply in proportion to the probable traffic and unit of tariff, and, indirectly, from analogically situated establishments. In this case the researches prove that the proportion between the tariffs, which are to pay the interest on the capital and the cost or maintenance and working, may be expressed as follows:—*The normal tariffs are in direct proportion to the square of the distances and in inverse proportion to the number of commercial units.*

This rule applies to direct communications for special traffics, and to lines in which the cost of establishment per mile is the same. It is easy to see that the ratio at which the tariffs under the stated suppositions increases in proportion to the distance is correct;

if, for instance, the number of commercial units remains the same, but the proposed line is double the length of the line taken as standard, then the probable traffic will amount to only half of the actual traffic, whereas the proposed line will require double the capital of the standard line. If, therefore, the financial position of both establishments shall remain the same, the proposed tariff must in this case be equal to four times the tariff employed on the standard line. It will be observed that about this ratio of tariffs is likely to come in force between the proposed telegraph across the Pacific Ocean and the Transatlantic telegraphs, the latter taken as standard, and provided, further, the commercial traffic across the Pacific Ocean be about one-sixth of the commercial traffic across the Atlantic. If the former commercial traffic be less, then the tariff increases in proportion.*

The tariffs, or rather ratio of tariffs, thus arrived at, are, of course, the theoretical consequences of the traffic-law. It is true that the reality seldom answers to the state of things as simply and clearly as the theory provides, but a theoretical determination of the tariff will, nevertheless, always remain a guide for the tariff to be finally settled, so much the more as the former can be constructed without being exposed to complicated considerations.

There are certain observations to be made in regard to the conditions of the commercial traffic along the line from which the theoretical tariff is deducted. In the case of the Atlantic Telegraph the tariff is not only influenced by competition, but particularly by a considerable *regular* traffic of steamers, which tends to favour the development of the small trade, and therefore an intensity of the telegraphic traffic, which cannot be expected to be met with across the Pacific. The tariff, deducted from the Atlantic Telegraphs, is therefore to be regarded as a minimum. The statistics furnish no accounts by which to establish the proportion of tariffs to the general commerce; a small table,† calculated from statistical statements and given in the treatise, may therefore serve to form, for several countries, an idea of the proportion of their telegraph expenses to the value of the trade. From this table it appears that the

* Recherches sur la loi, &c., page 49. † Ib., page 52.

expenses of international telegrams are in percentage of the total value of the foreign trade as follows:—

For Denmark	. . .	0·18 per cent.
„ Sweden	. . .	0·22 „
„ Norway	. . .	0·38 „
„ Australia	. . .	0·21 „

The exchange of international telegrams costs, therefore, about one-fourth per cent., or, when the participation of the native merchant fleet in the foreign and remote navigation is considerable, at the utmost one-third per cent. of the value of the total commerce. This amount may certainly be termed a very small duty, particularly when compared with all other commercial expenses, and with the service rendered to commerce and navigation—a service, the value of which can scarcely be estimated, and which lends the telegraph the character of an insurance institution for the whole international traffic.

From what appears in the part of the treatise concerning the general *international* telegraph tariffs, the significant circumstance may first be named that the effect of the international telegraph tariffs has not been observable in the researches, and that therefore the tariff does not form a factor of its own, but is only *implicitè* represented in the factor p, which contains, besides, several other elements. This fact is the more characteristic and significant, as there can be no doubt that the *inland* telegraph tariff most actively partakes in the development of that traffic. The treatise demonstrates that the whole amount of progress in the factor p for a period of 18 years cannot at an average be calculated to be more than 100 per cent. Supposing that all of this may be ascribed to the progress in the telegraphic organisation only, then at least 50 per cent. are due to technical and administrative improvements, and the rest of 50 per cent. may then be said to cover the effect of the constantly reduced tariffs. That is to say, the reduction of the international telegraph tariffs has increased the telegraph traffic, *due to the same amount of commercial traffic*, in the proportion of from 100 to 150 at the utmost, whereas the general reduction of the tariffs in the same time is certainly not less than from one to one-fifth. We are, therefore,

no doubt right in the conclusion that the onward movement in the *intensity* of the traffic is not so much the effect of modifications of the tariffs as in a far higher degree the effect of the incessant improvements in the service itself, and of the constantly increasing *safety* and *rapidity* in the transmission of the messages.

The independence of the intensity of the traffic (the co-efficient C) in regard to the tariff is also proved by the fact that the co-efficients do not in any way arrange themselves parallel with the rates; we find on the contrary that a co-efficient of the highest class, representing the most intensive traffic (the Danish-Norwegian traffic), includes a rate of nearly the same amount as rates which come within co-efficients of much lower order.*

When the tariffs are settled with the view of yielding the necessary moderate funds for the maintenance, renewal, and extension of the telegraph system now in existence, so as to keep pace with the growing traffic and with the growing demands of the public, the question of regulation of the tariffs may be considered from another point of view.

It is (namely) so that a trade of a value of (say) one million pounds may be divided either in so and so many thousand parts or only in a few hundreds. In the first case the trade will produce a much larger amount of telegrams and letters than in the last. Provided now that the trade, in either case, is to pay the same rent for the use of the telegraph, it follows that the tariff should be settled in proportion to the division of trade, consequently so that the more detailed commercial traffic, disposing of a larger amount of telegrams, is to be served with a reduced tax, and a more concentrated traffic with a higher tax. It is from this point of view that the commercial organisation is of special interest for the administrations of telegraphs. In the absence of official statements, I have in the "New Researches" tried to come as near as possible to the question at issue. The table thus constructed confirms what reasonably must be expected to be the case, that the trade between the nearer countries has much more the character of a small trade than the trade with remoter countries. The researches and considerations establish, therefore, the law of distance for the telegraph

* Recherches sur la loi, &c., page 54.

tariffs, the natural consequences of the formation of the traffic itself. This disposition of the general international telegraph tariffs is, moreover, in perfect consistency with the facts, that each telegram is an individual requiring a particular manipulation from beginning to end, and that each telegram disposes of capital and work, particularly required and at disposal for this service only, in proportion to the mileage of transmission.

It will not be possible to determine the exact effect on the traffic of modifications of the tariff before the traffic-law has been more fully worked out and has acquired a higher degree of completeness, but from the above it is probable that alterations in this part of the factor p, but to a very limited extent, will be able to modify the value of the co-efficients.

There remains now to be mentioned the application of the law as a means whereby to state under *normal* circumstances the efficiency of the organisation of the international telegraph, periodically instituted by the triennial Telegraph Conferences. The present organisation came in force at the beginning of 1876 by the St. Petersburg Convention; the status of the organisation at the end of 1875 is consequently the basis for the development of the telegraph for the present period, and thus we have the equation :—

$$T_{1875+n} = \frac{c_{1875}(100 \pm p)^n}{100^n d}(U_{1875+n}) \quad . \quad . \quad (5)$$

By the aid of this equation the value of the factor $\pm p$ can be determined for each year and each pair of countries, and the average value will then prove whether the actual organisation has accelerated or retarded the movement as compared with the effect of the preceding organisation.

It would now be of interest to introduce other considerations in close connection with the subject, for instance, the dissolution of the great trade as the consequence of the present means of rapid communications, and the parallel movement which therefore seems to exist between high co-efficients, or great intensity of the traffic, and the frequent *regular steam navigation;* or, the raised potential of international life generally, the consequence of the telegraph shortening the time of negotiations, suppressing the *dead time* in all affairs, and thus concentrating in the same space of time a sum

of actions not before witnessed. However interesting, I must abstain from bringing these subjects before the Society, and so is the case with observations in relation to other moments, such as *the commercial line*, *the commercial organisation*, and *the commercial centres of gravity*, which no doubt would prove to be of interest, illustrating, as they do, how nature, the geographical situation, the configuration of the coast-lines, &c., exercise a distinct influence on the international movement, thus enabling us to define with more accuracy, and in figures, the geographical and commercial situation of a country. In these respects, as in so many others, the international telegraph traffic will be found to be a most valuable quantity, representing in few and comprehensive figures the totality of international intercourse, and thus rendering it possible to construct the equations we have considered. I venture to add that the commercial centres of gravity, determined by means of the international telegraph traffic, will be found to be of profound use and interest, partly on their own account and partly in respect to the accurate definition of the fundamental factor of the co-efficients the distance between the countries, the fundamental factor also of the law of tariffs, which naturally follows the law of traffics. No doubt the generalised law will require a long and careful study, earnest work, and thorough discussion from competent quarters, but at the same time the present researches evidently seem to prove, that a mathematical relation between important branches of the international traffic does exist, and that the law now defined is in perfect accordance with the geographical and commercial conditions and the general development of international communications of the countries considered.

The PRESIDENT: It is a novel treatment of an interesting question, and I hope Members will have some observations to make upon it.

Mr. LATIMER CLARK: It is a little difficult on first hearing the paper to form an opinion upon it. One must study it in detail before one can understand it. I will make one remark upon it. Mr. Madsen has shown distinctly, as it appears to me, the nature of the causes which create and affect the amount of telegraph traffic between different centres and kingdoms, but it appears doubtful to

me whether he has given sufficient importance to the tariff as a factor in his calculations, and I notice that he speaks of distance as being a diminishing cause. I think it is useless to criticise the paper more closely until one has had an opportunity of studying it in private and in detail.

The PRESIDENT: I wish to propose a vote of thanks for this very interesting communication. I am myself too little familiar with the question of traffic to form any opinion that would possess value on this subject. Still I conceive that Mr. Madsen has treated the subject with a great deal of thought and boldness, and I have no doubt we shall arrive at some general rules by which we may determine, on the one hand, the telegraphic traffic possible between two countries, the conditions of which we know, and *vice versâ*, the condition of the countries, from the traffic which they have afforded. I need not point out how interesting it will be for those who wish to establish international communication, if such a law can be determined, and I think our best thanks are due to Mr. Madsen for bringing this subject before us.

A vote of thanks was unanimously accorded to Mr. Madsen.

The Secretary then read the following paper—

ON THE UNIT OF THE BIRMINGHAM WIRE GAUGE.

By C. V. WALKER, F.R.S. F.R.A.S. Past-President.

THE want of a standard of reference for the Birmingham Wire Gauge has long, I might almost say has always, been felt. Hardly any two authorities are in agreement; and the authorities are many. The more one examines the Tables that are severally put before us, the more one is troubled to discover the basis upon which they are constructed. My purpose in this communication is to discuss the figures that are engraved upon a gauge in my possession, which have led me to conclude that, if I have not before me *the* standard, I have at least *a* standard, of which the values are calculated upon a much more definite system than is to be found, as far as I can see, elsewhere. If this is not the long-lost standard, it certainly possesses materials that might profitably

be turned to account towards establishing an authorized gauge. It is not my intention to propose a gauge, but merely to set before the Society in due form the values, as I find them, upon this particular gauge.

In the year 1860, Messrs. R. Johnson and Nephew, now of the Bradford Iron Works, Manchester, placed in my hands a pocket B. W. G.; and shortly afterwards at my instance they prepared and kindly presented a copy of the same to the Astronomer Royal.

On the silver cover were engraved five columns of figures: the first column gave the B. W. G. wire numbers, and the second gave their respective diameters in FRACTIONS of an inch. Our present concern is with these two columns only. The B. W. G. Nos. and their diameters extend from No. 5/0 to No. 30; except that Nos. 19, 21, 23, 25, 27, and 29 are wanting. The gauge which is before you has two blades, one of which extends from No. 1 to No. 15, the other from No. 16 to No. 42, wanting none. It is provided also with a slide gauge, one side of which gives 3 inches (the total length of the gauge), each inch sub-divided into 32 parts. The opposite side gives Nos. 0 to 6/0. The weight for the pocket is 3 oz. The gauge was made by Mr. Thomas Mallinson, fine-wire drawer, until lately of Ashter Row, Birmingham.

With the gauge I received a leaflet, printed on one side only, with the gauge numbers and their diameters. It was highly valued by the owners, and I was requested to return it; which I did, having first made a copy of the figures. A manuscript before me, which I believe is this copy, extends to No. 32.

The conclusion to which I have arrived, from examination of the figures that were before me, is that the unit of this copy of the B. W. G., which I have ventured to name the C. V. W. unit, is $\frac{1}{640}$th in. (one six hundred and fortieth part of an inch.)

The diameter of a 1-inch rod is therefore 640 of these units. I have extended the table downwards, on principles to be explained, from No. 30, the last engraved No., to No. 46. This extension is purely tentative or suggestive. It brings No. 46 to be *one* unit.

Col. 1 of Table I. gives the Nos. complete of a gauge from 1 inch to No. 46.

Col. 2 contains the diameters as I find them engraved on the

silver cover, together with the first three which are above the ruled line, and which are taken from the slide gauge; but the values for Nos. 3/0, 0, 14, 17, 20, and 26 are given corrected; the errors of which, that were on the original, will be explained in due course.

I have reduced the fraction values of col. 2 into C. V. W. units in col. 3.

Col. 6 shows the diameters, as they are incorrectly given on the original, of the six Nos. just mentioned as being given corrected in col. 2.

Col. 7 gives the result of reducing these errors into C. V. W. units. It is seen at a glance that they are incorrect, for they would be out of place in the series of figures in col. 3—for they would break the evident uniformity. If this view required confirmation it is to be found in a Table given by Col. Tal. P. Shaffner, at p. 523 of his Telegraph Manual, published in 1859. His Table extends only from No. 1 to No. 16. The values are given in fractions, and when reduced are identical with those resulting from my engraved values. His series includes only one of my false values, No. 14, which he gives as $\frac{7}{80}$, as I had found it necessary to do in my corrected value. The authority for his Table and my gauge are evidently identical.

Col. 8 gives only the wire numbers that appear on my gauge. Blanks show the Nos. wanting.

With these corrections the series of values is complete only as far down as No. 30 or 32. Beyond this I was without a guide. The B. W. G. was established long before the origin of the electric telegraph, very long before there was much if any demand for wire finer than No. 32, or 8 C. V. W. units, or $\frac{1}{80}$ inch; and I doubt very much whether in those distant days any values had been worked out for finer wires, at any rate on the basis of the B. W. G. I note, for instance, that in the "Experiments with a Constant Voltaic Battery" on its heating powers, made by myself and Mr. Gassiot in 1838-9, we speak of the platinum wires used as $\frac{1}{50}$th, $\frac{1}{100}$th, $\frac{1}{200}$th of an inch in diameter, not as No. this or that of the B. W. G.

In the absence of any clue to the principles upon which the dif-

ferences of diameters had been determined, and upon which the transition from one value to another value of differences had been made, I gave up the quest almost in despair in 1860-1, and put all the papers aside. I have only recently taken up the question again, nor can I say that much greater success has attended my inquiries; for, as I have already mentioned, the extension of the table downwards as now shown is but tentative and suggestive. There are, however, two or three features in col. 3 that merit consideration.

Taking the wires in groups as in Table I. we see that the differences between wire and wire are similar in a given group, greater among the larger wires, less among the smaller, as might be expected. And there is a clear relation between the differences themselves of the consecutive groups. They follow in the order of one-half, that is 40, 20, 10 in the higher numbers; 8, 4, 2, 1 in the lower. They are shown in brief in Table II. There are in all, with my extension downward of the gauge, and to which I will presently return, *ten* groups of differences. If taken in pairs, beginning with the extreme pair, then with the next pair, and so on, as shown in the table, the product of multiplication is the same throughout, namely 80. For instance, groups 1 and 10, $160 \times \frac{1}{2} = 80$; groups 5 and 6, $10 \times 8 = 80$.

My authority, as I have said, took me down only as far as No. 32, the end of my group 9, where the differences were 1 C.V.W. unit. Groups 2 and 9, 80×1, gave the constant 80. In order to obtain the same constant for group 1, where the difference is 160, the inference was, as I have put it, that the unit of difference for group 10 might be $\frac{1}{2}$.

But there is nothing to show upon what grounds the differences themselves were determined—why they should have been made 40 instead of (say) 30, or any other number in the leading group; and why the following group of differences should have been fixed at one-half, rather than one-third, or any other part of the difference found in the preceding group.

Nor is there anything to show why there should be an abrupt break of gauge between Nos. 11 and 12. The group-differences above No. 11 were 10, or multiples of 10; those next below are 8,

followed by sub-multiples of 8; the rule of halves within the groups being taken up again here, and followed to the end.

Then as to the length of the groups, that is, the number of wires or sizes in each. There is nothing to show why the groups successively should have been made to differ as they do in the order 4, 4, 8, 5, 4, 4, and 8. I have made the last group extend to 14 in all with half a unit difference. For any sufficient reason to the contrary I might have divided it at No. 40; this would have made the difference beyond No. 40 a quarter of a unit, which would have extended the series to No. 52, before arriving at a wire of 1 C. V. W. unit in diameter.

In my endeavours to find the place in the gauge for certain specimens of *existing* fine wires of given numbers, I knew of no better authority than Mr. Walter Hall, of Mansfield Street, Borough Road, London, S.E.; and accordingly applied to him for specimens of his Nos. 30, 35, and 40 copper wires, with their diameters according to his scale.

His values are dealt with in Table III. His B. W. G. Nos. in col. 1 are given in his mils. in col. 2, and reduced to C. V. W. units in col. 3, with the B. W. G. Nos. in col. 4, corresponding with the said units as given in Table I.

Col. 5 shows the actual measures of the said specimens, made with my gauge, and which closely correspond with Mr. Hall's Nos. in col. 1.

The coincidence between Mr. Hall's sizes and mine is encouraging, and goes far towards confirming me in the impression that I was not far wrong in assuming half a unit as the decrement from No. 32 downward, at any rate as far as No. 40.

In Fleeming Jenkin's "Electricity and Magnetism," p. 202, published in 1874, I found some values for Nos. 30 and 36 which are not so encouraging. No. 30 comes out as between Nos. 32 and 33 of my Table I.; and No. 36 comes out as No. 43 of the said Table.

The difference between cols. 4 and 5 may in part be due to there being a difference between the diameters assigned to the specimens and their actual diameters. Equal lengths of the three wires were therefore taken and weighed. The ratio between the diameters as

assigned is 1·0 : 1·54 : 2·54. The ratio between the diameters obtained from the *weights* of the respective specimens is 1·0 : 1·45 : 2·37. I should add that No. 30, of col. 1, was a large No. 30 of col. 5. This Table shows how very nearly Mr. Hall's sizes coincide with mine.

A part also of the differences must be due to the slots in the blade of my gauge not corresponding accurately with the dimensions given on the silver cover.

The gauges themselves are indeed a fertile source of error. Col. 6, for instance, which differs greatly from col. 5, gives the measures of Hall's wires taken from another gauge that was at hand. The question of the gauges themselves has recently been taken up in America.

The "Journal of the Franklin Institute" for February, 1878, contains a "Report on a Standard Wire Gauge" of six pages, read before the *American Institute of Mining Engineers*, at the Amenia Meeting, October, 1877, signed by J. Ecleston, Chairman, William Metcalf, and Jos. D. Weeks.* They dwell mainly upon the gauges themselves, which they say should be simple in construction, not readily worn, easy of adjustment, and not too expensive. Reference is made to (1) those like our ordinary gauge, made with slots, and the sides of which they state are not always parallel, as of course they should be; (2) those with holes, as the Whitworth wire-gauge; (3) those of a V, cut in steel, or of two steel bars; (4) sliding calipers with verniers; (5) the micrometer screw-gauge; and they decide in favour of the latter, with the sizes to be expressed in thousandths of an inch [or mils.]; elsewhere spoken of as fractions of an inch.

They found, as we do, that gauges "not only differ according as they are made by different manufacturers, but in a package of a dozen, made by the same manufacturer, there were often very perceptible and annoying differences. . . . Neither the number nor the diameter is ordinarily correct, so that there is a double source of inaccuracy, as the number does not express the exact diameter, nor the diameter the number." But they give no copies of numbers and diameters for any individual gauge, which is the question now before us.

* See Abstracts and Extracts.

Mr. Latimer Clark, who has long been concerned at the confusion that reigns, took the question in hand some years ago, and presented a paper "On the Birmingham Wire Gauge" to the *British Association*, which was read at Dundee in September 1867; followed by another, read at Exeter in August 1869. He has himself printed them in extenso, 20 pp. 8vo. I hope he may allow them to be reproduced, and so preserved among the "Original Communications" in the Journal of our Society.

In his former paper he speaks of the origin of the system and its date being unknown; of there being no authorized standard in existence; and of a great number of gauges being in practical use, which differ from each other to a serious extent.

He has prepared a "Table of the sizes of the Birmingham Wire Gauge," as given by different authorities [thirteen in all], "diameters in mils. or thousandths of an inch." If all other evidence were wanting, this Table alone bears witness to the fact, put on record by Mr. Clark, that there is no authorized standard in existence. It is not necessary to reproduce the Table complete here. But a few illustrations of the sizes assigned by the several authorities to certain wires familiar to us have been extracted from Mr. Clark's Table, and reduced to C. V. W. units in Table IV. It will be seen that Warrington, Culley, and Rylands, 1862, are alike throughout; they are alike also in the complete Table, and are doubtless derived from the same source, and may therefore be counted as one authority. There are no less than nine different sizes attributed to our old familiar wire No. 8, and not one of them corresponds with the Johnson and Nephew's size, which is a trifle higher than any. If I were in want of a unit for a new scale or system, I should avail myself of No. 11. The value assigned to it, one-eighth of an inch, is very consistent, and it has the majority of advocates. It comes out in a whole number, 80 C. V. W. units, or 125 mils. And this same value is given to it by the Postal Telegraph Department. This is seen in col. 2 of Table V., which is taken from p. 178 of Preece and Sivewright's "Telegraphy," published in 1876. Col. 3 gives the mils. of col. 2 in C. V. W. units. Col. 4 actual values in C. V. W. units to the respective wires, numbered in cols. 1 and 5.

With so many conflicting elements before him, which it was impossible to reconcile, Mr. Clark expressed in his paper a hope that the British Association might appoint a Committee, and "issue a gauge under their authority, bearing the title of the British Association gauge, or British gauge." Failing this, he took the matter in hand himself. He was "inclined to think it probable that the original gauge may have been formed by taking as its basis No. 16 bell-wire, having a diameter of $\frac{1}{16}$th of an inch, and that each succeeding size up to No. 1 was formed by successive additions of 25 per cent. to the weight. This would be equivalent to successive increments of 11·8034 per cent. to the diameters . . . ." He expressed the several values in mils. or thousandths of an inch. He has not taken 62·5 mils., which is the actual equivalent of $\frac{1}{16}$th of an inch, but, "by assuming 65 as the diameter of No. 16 wire, and forming the other sizes from it, by constant increments of 25 per cent. in weight (or, what is the same thing, by constant decrements of 20 per cent. in weight) he obtained the values given in col. 5 of Table I.; and which I have there reduced to the C. V. W. units of col. 4, in juxtaposition for comparison with the actual units in col. 3.

It should be noted that in all these Tables where the values are shown in mils. the absolute values are mostly wanting, because all decimal figures beyond the first one are dropped. It is characteristic of the values on the gauge, which forms the subject of this communication, that they are all definite and absolute, and come out in whole numbers, sub-multiples of our old friend the inch, which, with its halves and quarters and eighths and sixteenths, is so familiar to the "shopkeepers and ordinary workmen" mentioned by Mr. Clark (p. 7). In contrast with the values there shown in col. 3 of Table I. I have prepared Table VI., in which they are reduced to mils., retaining all decimals.

I could have contributed Tables of diameters from several other authorities, but they are of the same general and indefinite character, and could contribute in no way towards the solution of the difficulties in which we are involved. They make our present confusion worse confounded, and are better not produced. The merit of the gauge to which the attention of the Society is now called

is its simplicity, at least its claim to a larger degree of simplicity than has any one of its rivals. Its weak points have been indicated. The obscurity in which are buried the principles upon which its uniformity and want of uniformity are based have not been overlooked.

While we have the true time and the standard foot and yard and pound weight, open to all the world for reference on the outer walls of the Royal Observatory, Greenwich, it is almost absurd to say that we have no means of knowing for certain what the diameter of an article in such common use as No. 8 iron wire ought to be.

TABLE I.

Diameters of Wires—B. W. G. in C.V.W. Units.

Johnson & Nephew's Gauge.			Latimer Clark's Gauge.		Engraver's errors.		
1. Nos.	2. Diameters.	3. C. V. W. Units.	4. In C. V. W. Units.	5. In Latimer Clark's Mils.	6. Fractions.	7. C. V. W. Units.	8. Nos.
in.	in.						
1	1	640					
7/0	$\frac{3}{4}$	480					
6/0	$\frac{5}{8}$	400					
5/0	$\frac{9}{16}$	360	387·3	605·3			5/0
4/0	$\frac{1}{2}$	320	346·4	541·4			4/0
3/0	$\frac{14}{32}$	280	309·9	484·3	$\frac{15}{32}$	300	3/0
2/0	$\frac{3}{8}$	240	277·1	433·1			2/0
0	$\frac{11}{32}$	220	247·9	387·4	$\frac{1}{3}$	213·$\dot{3}$	0
1	$\frac{5}{16}$	200	221·7	346·5			1
2	$\frac{9}{32}$	180	198·3	309·9			2
3	$\frac{1}{4}$	160	177·4	277·2			3
4	$\frac{15}{64}$	150	158·6	247·9			4
5	$\frac{7}{32}$	140	141·8	221·7			5
6	$\frac{13}{64}$	130	126·9	198·3			6
7	$\frac{3}{16}$	120	113·5	177·4			7
8	$\frac{11}{64}$	110	101·5	158·7			8
9	$\frac{5}{32}$	100	90·8	141·9			9
10	$\frac{9}{64}$	90	81·2	126·9			10
11	$\frac{1}{8}$	80	72·6	113·5			11
12	$\frac{9}{80}$	72	64·9	101·5			12
13	$\frac{1}{10}$	64	58·1	90·8			13
14	$\frac{7}{80}$	56	51·3	80·3	$\frac{1}{12}$	53·$\dot{3}$	14
15	$\frac{3}{40}$	48	46·4	72·6			15
16	$\frac{1}{16}$	40	41·6	65·0			16

TABLE I.—*continued.*

Johnson & Nephew's Gauge.			Latimer Clark's Gauge.		Engraver's errors.		
1. Nos.	2. Diameters.	3. C. V. W. Units.	4. In C. V. W. Units.	5. In Latimer-Clark's Mils.	6. Fractions.	7. C. V. W. Units.	8. Nos.
17	$\frac{9}{160}$	36	37·1	58·1	$\frac{1}{18}$	36·$\dot{7}$	17
18	$\frac{1}{20}$	32	33·2	52·0			18
19		28	29·7	46·5			19
20	$\frac{3}{80}$	24	26·6	41·6	$\frac{1}{25}$	25·6	20
21		22	23·8	37·2			21
22	$\frac{1}{32}$	20	21·3	33·3			22
23		18	19·0	29·7			
24	$\frac{1}{40}$	16	17·0	26·6			24
25		15	15·2	23·8			
26	$\frac{7}{320}$	14	13·6	21·3	$\frac{1}{510}$	14·4	26
27		13	12·1	19·0			
28	$\frac{3}{160}$	12	10·8	17·0			28
29		11	9·7	15·2			
30	$\frac{1}{64}$	10	8·7	13 6			30
31		9	7·8	12·2			
32		8	6·9	10·9			
33		7½	6·2	9·7			
34		7	5·5	8·7			
35		6½	4·9	7·8			
36		6	4·4	6·9			
37		5½	3·9	6·2			
38		5	3·5	5·6			
39		4½	3·2	5·0			
40		4	2·8	4·5			
41		3½					
42		3					
43		2½					
44		2					
45		1½					
46		1					

TABLE II.

Units of Differences within the Groups of Wires.

	B.W.G. Nos.	C.V.W. Units.
Group 1	1 in and 7|0	differ by 160
„ 2	7|0 „ 6|0	„ „ 80
„ 3	5|0 to 2|0	„ „ 40
„ 4	No. 0 to No. 3	„ „ 20
„ 5	„ 4 „ „ 11	„ „ 10
„ 6	„ 12 „ „ 16	„ „ 8
„ 7	„ 17 „ „ 20	„ „ 4
„ 8	„ 21 „ „ 24	„ „ 2
„ 9	„ 25 „ „ 32	„ „ 1
„ 10	„ 33 „ „ 46	„ „ ½

× =80

TABLE III.

Walter Hall's Fine Wires.

As given by Hall.		Reduced to		By Measure.	
1	2	3	4	5	6
				C.V.W.Gauge	Spare Gauge.
Nos.	Mils.	C.V.W.-Units	Nos.	Nos.	Nos.
30	14	8·96	31	30	27
35	8·7	5·56	37	35	30
40	5·5	3·52	41	40	39

TABLE IV.

DIAMETERS IN C. V. W. UNITS OF CERTAIN SELECTED WIRES AS GIVEN BY VARIOUS AUTHORITIES.

Authorities.	No. 6.	No. 8.	No. 11.	No. 14.	No. 18.	No. 22.	No. 35.	No. 40.
C. V. W. Unit	130	110	80	56	32	20	6½	4
L. C. . .	126·9	101·5	72·6	51·3	33·2	21·3	4·9	2·8
Warrington .	128	108·8	80	54·4	32	19·2		
Silvertown .	124·1	103						
Culley . .	128	108·8	80	54·4	32	19·2	5·0	2·1
Rylands, 1862 .	128	108·8	80	54·4	32	19·2		
Rylands, 1866 .	128	101·7	74·8	50·5	30	17·9		
Holtzapffel .	129·9	105·6	76·8	53 1	31·3	17·9	3·2	
Molesworth .	128	106	80	53·1	31·3	17·9	3·2	
Schaw, R E. .	133·3	109·9	80	56	33·3	19·9		
Cocker . .	128	102	76·8	57·6	32	19·2	3·8	
Bartholomew .	133·7	107·8	80	54·4	33·9	19·2	3·5	
Whitworth .	128	105·6	76·8	54·4	32	17·9	.	
Hall . .					27·5	20·4	5·5	

TABLE V.

Preece and Sivewright's Diameters of certain Wires.

(Telegraphy, p. 178.)

	Preece and Sivewright's Values.		C. V. W. Units.	
1	2	3	4	5
Nos.	In Mils.	In C.V.W. Units.		Nos.
4	240	153·6	150	4
5	220	140·8	140	5
6	200	128·0	130	6
7	180	115·2	120	7
8 G. P. O.	170	108·8	110	8 G. P. O.
8	165	105·6	110	8
9	150	96·0	100	9
10	135	86·4	90	10
11 G. P. O.	125	80·0	80	11 G. P. O.
11	120	76·8	80	11

TABLE VI.

C. V. W.—Units of Table I. in Mils.

Nos.	Mils.	Nos.	Mils.
1 in.	1000	17	56·25
7|0 = $\frac{3}{4}$	750	18	50·0
6|0 = $\frac{5}{8}$	625	19	43·75
		20	37·5
5|0	562·5	21	34·375
4|0 = $\frac{1}{2}$ in.	500·0	22 = $\frac{1}{32}$ in.	31·25
3|0	437·5	23	28·125
2|0	375·00	24	25·0
0	343·75	25	23·4375
1	312·5	26	21·875
2	281·25	27	20·3125
3 = $\frac{1}{4}$ in.	250·0	28	18·75
4	234·375	29	17·1875
5	218 75	30	15·625
6	203·125	31	14·0625
7	187·5	32	12·5
8	171·875	33	11·71875
9	156·25	34	10·9375
10	140·625	35	10·15625
11 = $\frac{1}{8}$ in.	125·0	36	9·375
12	112·5	37	8·59375
13	100·0	38	7·8125
14	87·5	39	7·03125
15	75·0	40	6·25
16 = $\frac{1}{16}$ in.	62·5		

The PRESIDENT : I will ask Mr. Walker whether he has any observations to offer supplementary to the paper.

Mr. C. V. WALKER: I will add just a few words in explanation of Table I. now before you. My communication is merely a report worked out from the figures of the gauge that I hold in my hand, including particulars of the several weak points which presented themselves in the course of my investigation. Before putting my papers aside in 1860-1, I had found that nearly all the given fractions could be reduced to one common denominator, viz., the 640th of an inch; and easily detected those which could not but be errors; they were so conspicuous that I felt perfectly justified in substituting the corrected values.

Take for example No. 0; the proper, that is the corrected, value is 220. The value I found there, when reduced, came out 213·3. Now, the value for No. 2 | 0, the next above No. 0, is 220 + 20 = 240; and for No. 1, the next below No. 0, is 220 − 20 = 200; and Nos. 2 and 3 which follow also differ by 20 respectively. But the false value, 213·3, would have given as differences 26·7 and 13·3 respectively, which is consistent with the order that prevails generally. Look again at No. 14, the proper value of which is 56, and in a place or group where the differences are 8. But the false value is 53·3′, which produces 10·7 and 5·3′ respectively as differences, which is also inconsistent; and so of the other quasi-values in cols. 6 and 7.

It was satisfactory to find the series of numbers in col. 3 following some definite system. But the weak point—the one missing link in the system, and to which I have called attention, is this :—I can detect no reason why the differences should vary as they do. Take for instance the higher group, Nos. 5 | 0 to 2 | 0, which give 360, 320, 280 and 240, or differences of 40, and there are only four Nos. in that group. Then we turn to the next group, Nos. 0 to 3, a group also of four, in which the differences are 20; and on to the next, a group of eight, in which the differences are 10; then to the next, a group of five, in which they are 8. This was a break of gauge that confused one. Why, one asks, go for 40, 20, 10 to 8?

Another puzzling point was why the breaks should be at the

existing places on B. W. G. Nos. rather than at any other places? Why should there be breaks between 20 and 21 and 24 and 25 for instance? Why not between 26 and 27, or elsewhere than they are? I tortured these various numbers and twisted them to see if I could find a clue to lead me to the law on which the differences and the breaks of gauge were based. The only uniformity I could trace is shown in Table II., in which I give the number of units of difference in each group and the next that follows, and I find by multiplying them together in pairs, as I have done in Table II., that 80 is the product of each pair. Groups 1 and 10, $160 \times \frac{1}{2} = 80$; groups 2 and 9, $80 \times 1 = 80$; and so on, all 80. Having worked down as far as my authorities took me, that is, to No. 30 and No. 32, and not finding in any table to which I had access any guide or clue for estimating downwards till reaching a wire so commonly used as No. 40, I have taken, which I much wished to do, the liberty of suggesting a difference of half a unit, when getting below 32 and working with half a unit till arriving at C. V. W. unit, which was at No. 46 B. W. G. As I have said in my paper that there is no particular reason why I should have made group 10 a group of 14; nor why I should have broken it, say at No. 40, and have then advanced by a quarter of a unit till I got as far as 1 C. V. W. unit, which would have been at No. 52.

As I have said in my paper, it is strange that we have no authorised standard. Mr. Latimer Clark mentioned in his paper that the choice of one or other of two so-called Birmingham wire gauges would have made a difference of £8,000 in a certain contract in which he was concerned. I had arrived at the value which I have given for No. 40, before writing to Mr. Walter Hall for specimens and his diameters of his three wires—40, 35, and 30. Table III., which is not on the wall before you, shows how nearly Mr. Hall's values coincide with mine, which is encouraging, showing that I was not far wrong in the course I had taken. His values for Nos. 30, 35, and 40, correspond with my values for Nos. 31, 37, 41; the slots in my gauge make his numbers and mine identical. On the whole Table I. contains materials from which a standard wire gauge might be constructed, so that when we were dealing with No. 8 or No. 40, or any other number, we

should know what diameter we expected to find, and whether we had what we expected to have. The other columns of Table I. explain themselves. Column 5 contains Mr. Latimer Clark's values in his mils, and side by side in column 4 are his values reduced to C. V. W. units.

Mr. Latimer Clark: It might at first be thought that the confusion which exists among the wire gauges was a matter of little importance, as we all have or can easily obtain decimal gauges and express our meaning clearly in millimetres or in thousandths of an inch; in fact this is what we are compelled to do in all important matters. We specify both the B. W. G. and our interpretation of it: but the question will be asked if the decimal gauge answers so perfectly, why not employ it exclusively and drop the other; the very fact that this is seldom done by practical men points to the necessity there is for some standard wire gauge, for a recognised gauge offers a thousand conveniences both to the manufacturer and the consumer. The few sizes of the gauge are easily remembered by name and soon become familiar to the mechanic and to everybody who uses them. The shopkeepers can only afford to keep certain recognised gauges in stock, and the manufacturer is able to produce certain sizes of wire in large quantities, knowing that sooner or later the sizes he has made will be required. A wire gauge is, in fact, a practical necessity.

I have listened with much interest to Mr. Walker's paper, and having been favoured by that gentleman with an early copy of it, I have had the opportunity of giving it much consideration, and I have arrived at the same conclusion that he has, namely, that he has at last discovered the original basis on which the Birmingham Wire Gauge was formed. At the time I read papers on the subject before the British Association in 1867 and 1869,* I had not been able to form any opinion on this point, and I attributed the greatest probable degree of authenticity to Holtzapffel's table on account of the early date at which it was published; but subsequently I had seen reason from internal evidence to attribute more importance to the Warrington table from reasons which I will presently give.

* See Abstracts and Extracts.

I now, however, think it probable that Mr. Walker has solved the problem, and I think it is possible to conjecture with great probability how it came into existence. To make this more clear I have slightly altered the form of Mr. Walker's table and reduced the fractions given in his gauge to a common denominator, as will be seen on the table before you (see p. 234). Column 1 gives Mr. Walker's figures. In column 2 I have reduced all the fractions into 64ths of an inch, and in the lower half of the table to C. V. W. units, or 640ths of an inch. The last two columns we may neglect for the moment.

In my paper of 1869 I showed that the B. W. G. had evidently been formed in a very practical manner, and that the originators had probably taken a series of already drawn wires, calling the original rod No. 0 and the succeeding sizes No. 1,2,3, &c. I also showed that these sizes bore a natural relation to the physical properties of the metal itself. At first the conventional numbers probably came into use without definite measurements, each size representing a single draw. In order, however, to establish uniformity among the different manufacturers, it soon became necessary to give measurements of the different sizes, and the person who undertook this was probably a self-taught practical mechanician, who had little familiarity with decimal arithmetic. Accustomed to 8ths and 16ths of an inch, he would naturally turn to 32nds and 64ths, which have always been more or less in use. Measuring his series of drawn wires, he doubtless formed the table which Mr. Walker has brought to our notice, including the sizes from No. 1 down to No. 12. On looking at this table it appears pretty evident that the measurements have been made in 64ths, the first series down to No. 6° decreasing by 8, the second series down to 2° decreasing by 4, the third group decreasing by 2, and the last group decreasing by 1, as already pointed out by Mr. Walker; the number of sizes in each group being doubtless made to agree with the sizes of the actual specimens of wire before him. This brought him down to about No. 12 or 13, and it is possible that at first this was the intended limit of the table, the sizes decreasing by units down to a 64th of an inch.

As small wires came in course of time to be required, the im-

Johnson and Nephew's Gauge, Rectified by C. V. Walker.				The Warrington Gauge, Corrected by Henley.	
B. W. G.	Diameter in 64ths of an inch.		Diameter in mils.		
8°	64		1000		
7°	48	$\frac{8}{64}$	750		
6°	40		625		
5°	36		562·5	456	
4°	32		500	425	
3°	28	$\frac{4}{64}$	437·5	394	31
2°	24		375	363	
0	22		344	331	
1	20		312·5	300	
2	18	$\frac{2}{64}$	281	280	
3	16		250	260	
4	15		234·5	240	20
5	14		219	220	
6	13		203	200	
7	12		187·5	185	
8	11	$\frac{1}{64}$	172	170	
9	10		156	155	
10	9		140·5	140	
11	8		125	125	
	Diameter in 640ths of an inch.				15
12	72		112·5	110	
13	64		100	95	
14	56	$\frac{8}{640}$	87·5	85	
15	48		75	75	10
16	40		62·5	65	
17	36		56	57	
18	32	$\frac{4}{640}$	50	50	
19	28		44	45	5
20	24		37·5	40	
21	22		34·5	35	
22	20	$\frac{2}{640}$	31	30	
23	18		28		
24	16		25		
25	15		23·5		
26	14		22		
27	13	$\frac{1}{640}$	20		
28	12		19		
29	11		17		
30	10		15·6		

perfections of the system revealed themselves. It became evident that decrements of a 64th of an inch were too large for the smaller sizes of wire, and the question was how could they be further subdivided? 128ths and 256ths of an inch were unheard of fractions, and 512ths, 1024ths were equally so. Had he *weighed* his specimens, he would have found each size weigh just about 25 per cent. more than the preceding size, but this idea of weight unfortunately did not occur to him. Had the increase been 26 per cent., he would have found each third gauge double the weight throughout the scale. The only possible escape from the difficulty was to divide his 64ths by 10, and make them into 640ths, or C. V. W. units. Starting on this basis, he went over the same ground as before, decreasing first by 8 then by 4, 2, and 1, and his table would naturally have terminated with No. 39. $\frac{1}{640}$ths of an inch in diameter, although Mr. Walker suggests a still further subdivision. This I conceive to be the probable manner in which the table came into existence.

Passing from this part of the subject, I would now draw attention to some of the gauges at present in use. In my table column 3 gives the diameter of Mr. Walker's sizes in thousandths of an inch or mils, a name first introduced by Mr. Cocker, of Liverpool, and column 4 gives the sizes according to the Warrington gauge, as corrected by Mr. Henley, which is nearly identical with that of the original table of Messrs. Rylands Brothers, of Warrington,* and that of Mr. Culley, all of which are given in my paper of 1869, and are in very general use.

Now, on examining this gauge, it is evident that the unit is 1000th of an inch, and that it has been formed on somewhat similar principles to the Walker or Johnson gauge. Commencing at No. 1, it decreases by uniform intervals of 20 mils; at No. 7 by 15 mils; at No. 14 by 10 mils; and No. 17 by 5 mils; the lowest size given is No. 22. At the commencement of the table the 0 sizes increase by 31—but I imagine that this series was added subsequently, and did not form part of the original table; with the exception of these last-named numbers, it will be seen that the two tables agree in their sizes very closely throughout, and are evidently derived from

* Electrician, vol. i. p. 191, 1862.

the same source. Although the Ryland's table is so systematically formed, I imagine that it is of later date than the Walker or Johnson table, and that it was really derived from it, and formed at no very distant date, by actually measuring the sizes of the older gauge in thousandths of an inch, and then adjusting these sizes among themselves as uniformly as possible on the system of equal decrements.

There must be persons in the manufacturing counties who know something of the history of the B. W. G., or who have access to records concerning it; and it would be of great interest if they would make known the date and circumstances of its first introduction.

Let us come, however, to the practical part of the question. There are at least twenty different gauges in existence; is it possible to do away with this confusion and diversity? The question is by no means an unimportant one, for I have myself prepared the specification of a contract in which the use of one or another gauge would have made a difference of £8,000. Of course all difficulty was avoided by giving both the gauge and the size of the wires as is now usually done in all important contracts; but this necessity only proves the uselessness of the present system.

Is the Society prepared to take the matter in hand and recommend to the world a gauge for universal adoption? I believe such a step would be welcomed by the consumers, and would in a short time be equally appreciated by the manufacturers. As telegraphists are perhaps the largest consumers of wire, it is a function which this Society might with great propriety undertake, and the members by their own influence might, to a great extent, ensure its practical adoption.

There are at least two good practical gauges before us this evening. There is that brought to our notice by Mr. Walker, which has strong claims to be considered the original B. W. Gauge, and which fairly represents an average among all the gauges at present in use; its gradation, however is irregular, and it rests on no scientific basis.

The gauge I have proposed under the name of the "British Gauge," agrees almost equally closely with the gauges at present

in use, and might be substituted for them with equal facility; the diameters when plotted form a uniform logarithmic curve, increasing by 11·8034 per cent. (log. 2·0484472) which may be extended in either direction indefinitely; it has the advantage that each size weighs exactly 25 per cent. more than the succeeding or next smaller size, or what comes to the same, 20 per cent. less than the preceding or next larger size. Moreover it is not specially based on English measures, and is therefore as well suited for adoption in foreign counties as in our own. The English gauge is acknowledged by foreign writers to be the best adapted for practical use, and if used in England this gauge would doubtless become universal throughout the world. There might, however, be an advantage in adjusting the basis so as to make it coincide more nearly in the No. 8 and 11 gauges with the sizes used by the Government Telegraph Department. Other gauges deserve consideration; but in order to obtain the introduction of any gauge it would be necessary first to communicate with wire manufacturers and consumers and obtain their adherence. It would be also be necessary to issue a report on the subject and a description of the gauge recommended, and of its advantages. It would also be desirable to induce some manufacturer to produce, in the first instance, a certain number of carefully made standard gauges, which might, if it were thought desirable, be examined by and bear the impress of the Society. I am one of those who feel that this Society would gain much credit by taking the subject in hand, and would be amply rewarded for any trouble it might take in the matter.

I am sure we must all feel that our best thanks are due to Mr. Walker for bringing so important a subject under the notice of the Society.

Mr. WALTER HALL: Doubtless Mr. Latimer Clark is aware that I have taken this subject in hand for I think twenty years, and the first formula I published was in 1861. About this time Mr. C. Varley introduced "Cocker's" gauge, for ascertaining the diameter of copper wire; but it has not, to my knowledge, been used as a standard gauge. I think for three years at least I was groping in the dark, before arriving at a proper method for finding the true diameter of a copper wire, that would enable me to calculate its

resistance and conducting power—as I considered gauges useless for this purpose. In 1865 I calculated a table for that special purpose. My first object was to find the specific gravity of copper, which would enable me to calculate the diameter of a wire made from it. Having found the specific gravity to be about 8800; I arrived at the following formula for finding the diameter

$$d = \sqrt{\frac{\cdot 25 \times W}{L}}$$

where (d) is the diameter, (W) weight in ounces, and (L) length in inches. From this formula the following tables were calculated.

Electrical Resistance of Pure Copper.

The conducting power of pure copper is taken at 100, and the following calculations are made in accordance with the British Association unit of resistance, which is equal to one mile of pure copper of a diameter (D) — ·2302 in. and $D^2 = \cdot 0530349$. Thus, when $d^2 = D^2$, $R = 1$ unit in electro-magneto measure, as decided upon by the Committee appointed by the British Association for the standard unit of electrical resistance.

Table I. clearly shows that one mile of pure copper wire, No. 16 B.W.G. (diameter ·0625 in.) is = 13·59 B.A. units of recent determination. The value of C = 1·9th or ·111111.

This table, as will be seen, is well adapted for comparing the resistance of any size of copper wire corresponding to any diameter therein given, aud so the conducting power of such wire under test is easily ascertained, assuming a correct method of testing be adopted. In my opinion the instruments likely to give the most accurate results in measuring the resistance of copper wires are the Wheatstone bridge arrangements, or the one recommended by Mr. F. Jenkins, in his paper read before the British Association at Bath, in September last, known as Professor Thomson's Electric Resistance Balance. The reader may also find a full description of another excellent instrument for this purpose in the thirty-second report of the British Association, page 159. The manufacturers are Messrs. Elliott Brothers, of the Strand.

Table I.

$d = \sqrt{\frac{c}{l}}$ diameter.	$d^2 = \frac{C}{l}$	$R = \frac{D^2}{d^2}$ { Calculated resistance in B. A. units of pure copper.	$l = \frac{C}{d^2}$ { Number of yards in 1 lb. of wire.
·2302	·0580349	1·0	2·095
·226	·051076	1·038	2·175
·198	·039204	1·352	2·834
·183	·033489	1·583	3·317
·175	·030625	1·731	3·628
·160	·0256	2·068	4·34
·136	·018496	2·867	6·007
·128	·016384	3·237	6·781
·107	·011449	4·632	9·705
·10	·01	5·30	11·11
·092	·008464	5·266	13·125
·08	·0064	8·28	17·36
·07	·0049	10·82	22·67
·065	·004225	12·25	26·29
·0625	·0039025	13·59	28·472
·06	·0036	14·73	30·864
·058	·003364	15·76	33·03
·056	·003136	16·91	35·43
·054	·002916	18·18	38·104
·052	·002704	19·61	41·091
·05	·0025	21·21	44·444
·048	·002304	23·02	48·225
·046	·002116	25·06	52·51
·044	·001936	27·39	57·391
·042	·001764	30·06	62·988
·040	·0016	33·14	69·44
·038	·001444	36·72	77·16
·036	·001296	40·92	85·766
·034	·001166	45·48	95·292
·032	·001024	51·79	108·5
·030	·0009	58·93	123·46
·028	·000784	67·65	141·72
·026	·000676	78·46	164·36
·024	·000576	92·08	192·9
·022	·000484	109·58	229·56
·020	·0004	132·59	277·78
·018	·000324	163·69	342·94
·016	·000256	207·17	434·03
·014	·000196	270·58	569·51
·012	·000144	368·3	771·6
·010	·0001	530·35	1111·11
·0095	·00009025	587·64	1231·1
·009	·000081	654·75	1371·7
·0085	·00007225	734·05	1537·8
·008	·000064	828·67	1736·1
·0075	·00005625	942·84	1975·3
·007	·000049	1082·4	2267·6
·0065	·00004225	1225·3	2629·9
·006	·000036	1473·1	3086·4
·0055	·00003025	1753·2	3673·1
·005	·000025	2121·4	4444·4
·0045	·00002025	2619·0	5487·0
·004	·000016	3314·7	6944·4
·0035	·00001225	4329·4	9070·3
·003	·000009	5842·7	12346.
·0025	·0000625	8485·6	17777.

By way of explanation of the foregoing table, briefly:—

1. To find the diameter of any sized wire not in table, put d for the diameter; thus $d=\sqrt{\frac{C}{l}}$, l being the length in yards of 1 lb. of copper wire; or, $d=\sqrt{\frac{\cdot 111111}{28\cdot 472}}=\cdot 0625$, corresponding to one of the diameters in table.

2. But the diameters are much more easily found by logarithms, thus:—Log. $d=\frac{(\log.\ \bar{3}\cdot 8416347+\log.\ w)-\log.\ l,}{2}$ w being the weight in ounces, and l the length in yards.

3. The log. of $R=(\log.\ D^2-\log.\ d^2)$ therefore the log. of $R=(\log.\ \bar{2}\cdot 7245618-\log.\ d^2)=$ the resistance in B. A. units.

4. The Log. $l=(\log.\ C-\log.\ d^2)=(\log.\ \bar{1}\cdot 0457575-d^2)=$ the length in yards of 1 lb. of copper wire.

5. To find the conducting power (P) of copper when drawn into wire:—First, ascertain the diameter, by either of the above formulæ, then test for resistance (r), and the conducting power $P=\frac{R}{r}+100$. Example:—Find the conducting power of the metal in one mile of copper wire diameter, ·0625 in., having a re-resistance $(r)=16\cdot 5$ B.A. units; the value of R in table $=13\cdot 59$; therefore $P=\frac{R}{r}\times 100=\frac{13\cdot 59}{10\cdot 5}\times 100=82\cdot 36$; the conducting power of pure copper being 100. See Dr. Matthieson's elaborate "Report on the Electric Conducting Power of Metals, Phil. Trans. 1862."

Recently I have heard it stated that my formula was not correct. Referring to a book of Dr. Mathieson, on alloys, &c., I found the specific gravity of copper as 895; whilst Dr. Siemens took it at 891. I therefore made another calculation, and obtained the co-efficient ·246, instead of ·25, as given in my formula. I consider the latter however sufficiently near for all practical purposes.

I have always used this Table (*handing Table II. to the President*) for finding the diameter of fine wires, from the fact that I found it difficult to get more than an approximate measurement for any wire above No. 30 B.W.G., from the ordinary decimal gauge.

The capacity of pure copper is taken at 100, and the unit of

TABLE II.

TABLE OF RESISTANCES OF PURE COPPER WIRES.

Diameter of inch. $\left(d=\sqrt{\frac{C}{l}}\right)$	Diameter of m.m. $(=d\times 25\cdot 4)$	Number of yards. per lb. $\left(l=\frac{C}{d^2}\right)$	Number of metres in 1 kilo. $(=l\times 2\cdot 016)$	Resistance in ohms. of pure copper (unit of length 1760 yds. or 1609·31 mtrs.)
·2302	5·847	2·095	4·223	1·00
·226	5·740	2·175	4·384	1·038
·198	5·029	2·834	5·713	1·352
·183	4·648	3·317	6·680	1·583
·175	4·445	3·628	7·314	1·731
·160	4·064	4·350	8·75	2·068
·136	3·454	6·007	12·11	2·867
·128	3·251	6·781	13·671	3.237
·107	2·717	9·705	19·555	4·623
·10	2·54	11·11	22·398	5·300
·092	2·336	13·125	26·46	6·266
·08	2·032	17·36	35·00	8·288
·07	1·778	22·67	45·71	10·82
·065	1·651	26.29	53·00	12·25
·0625	1·587	28·472	57·40	13·59
·06	1·521	30·864	62·223	14·73
·058	1·473	33·03	66·588	15·76
·056	1·422	35·432	71·431	16·91
·054	1·371	38·104	76·818	18·18
·052	1·32	41·091	82·839	19·61
·05	1·274	44·444	89·60	21·21
·048	1·219	48·225	97·222	23·02
·046	1·168	52·51	105·86	25·06
·044	1·117	57·39	115·70	27·39
·042	1·066	62·98	126·96	30·06
·04	1·016	69·444	140·00	33·14
·038	·965	77·16	155·50	36·72
·036	·914	85·766	172·91	40·92
·034	·864	95·29	292·70	45·48
·032	·813	108·5	218·74	51·79
·03	·762	123·46	248·90	58·93
·028	·711	141·72	285·71	67·65
·026	·660	164·36	331·34	78·46
·024	·609	192·9	380·26	92·08
·022	·558	229·56	462·80	109·58
·02	·508	277·78	560·01	132·59
·018	·457	342·94	691·36	163·69
·016	·406	434·03	875·00	207·17
·014	·355	569·51	1148·10	270·58
·012	·305	771·60	1555·50	368·30
·01	·254	1111·11	2239·80	530·35
·0095	·241	1231·10	2481·90	587·64
·009	·228	1371·7	2765·30	654·75
·0085	·216	1537·8	3100 20	734·05
·008	·203	1736·1	3500·00	828·67
·0075	·190	1975·3	3982·20	942·84
·007	·177	2267·6	4571·00	1082·4
·0065	·165	2629·9	5300·00	1225·3
·006	·152	3086·4	6222·30	1473·1
·0055	·139	3673·1	7404·90	1753·2
·005	·127	4444·4	8960·00	2121·4
·0045	·114	5487·0	11062·00	2619·0
·004	·106	6944·4	14000·00	3314·7
·0035	·088	9070·3	18285·00	4329·4
·003	·076	12346·0	24890·00	5892·7
·0025	·063	17777·0	35838·00	8485·6

resistance used is the ohmad adopted by the Electrical Committee appointed by the British Association, one unit being equal to 1,760 yards, or 1609·31 metres of copper wire ·2302 inch diameter.

Rule for Obtaining Diameters.

d (diameter) $= \sqrt{\frac{C}{l}}$, l being the length in yards of 1 lb., or

$d = \sqrt{\frac{\cdot 111111}{28\cdot 472}} = \cdot 0625$, corresponding to one of the diameters in table. The value of C is 1-9th, or ·111111.

Or by another rule,

$$d = \sqrt{\frac{\cdot 25 \times W}{L}}$$

in which W represents the weight in ounces, and L the length in inches.

Formula for Obtaining Resistance.

$$P = \frac{R}{r} \times 100.$$

Example.

One mile of ·0625 copper wire has, say, a resistance, r, = 16·5 ohms.; the value of R in table = 13·59; hence

$$P = \frac{R}{r} \times 100 \frac{13\cdot 59}{16\cdot 5} = 82\cdot 36 \text{ per cent of pure copper.}$$

Referring to Table I, Mr. L. Clark's gauge, No. 30 size, corresponds very nearly to same size wire in my table, which I find copied in his paper on the Birmingham Wire Gauge, read before the British Association, at Dundee, 1867; and at Exeter 1869. I may add that all the sizes referred to by Mr. L. Clark were calculated from special samples of wire—but not having any gauge to guide me at the time—they are supposed to represent, as near as possible, their respective diameters, corresponding to the B. W. gauge.

Referring again to Mr. L. Clark's Table No. I., I find the diameter of No. 22, 26, 28, 30, and 32, to correspond very nearly with the

sizes of the Birmingham wire drawers. But if we compare the sizes, No. 33 (9·7 mils); No. 34 (8·7 mils); No. 35 (7·8 mils); No. 36 (6·9 mils); No. 37 (6·2 mils); and No. 38 (5·6 mils); with the Birmingham sizes, their gauges will be Nos. 34, 35, 36, 37, 38, and 40, respectively. I merely point this out, as regards the matter, when considered in a commercial point of view, in the event of reducing the diameter of the wires below the Birmingham standard. I would ask Mr. Walker what is the diameter of the No. 46.

Mr. C. V. Walker: No. 46 of my unit is the 640th part of an inch.

Mr. W. Hall: I fear that it would be difficult to construct a gauge which would give accurate measurements for such a fine wire that would enable the telegraph engineer to calculate the resistance and quality of copper under examination. I have myself a very beautiful gauge made by "Elliott Brothers" which is accurate enough for all practical purposes. But in all my experiments I always take 176 yards ($\frac{1}{10}$ mils.) of copper wire, and weigh it and calculate its diameter, then test for resistance; and lastly for conductivity (see Formula and Tables). In one of my experiments I found the gauge gave a diameter of ·006" (6 mils.), whereas the true diameter of the wire was ·00609" (6·09 mils.) by calculation, quality of copper under examination was 95 per cent., hence the difficulty experienced in using gauges for fine wire. I grant that a gauge is useful for large and medium size wires for telegraph and other purposes, but when it is necessary for the engineer to ascertain the quality of the copper wire, I think no better method can be adopted than that of obtaining the diameter from the formula herein described. At the same time I agree with Mr. C. V. Walker and Mr. Clark that a good standard gauge is indispensable for commercial purposes, in order to enable the London and Birmingham houses to draw their wires of one uniform size.

Mr. Lecky: I agree with Mr. Hall in one thing—that is, that the determination of diameter by weight would be useful; and if ever again there is a table of sizes made, I think the weights for certain lengths ought to be given with the gauge. There is one

point which I do not think has been touched upon. The Birmingham gauge was made exclusively for iron wire, and in my early days it was always called the B.I.W.G. It was afterwards applied to copper, but was never applied to steel. The numbers on steel gauges are different from those on iron, the numbers running in the opposite direction. On the former the smaller numbers indicate the finer wires. The Birmingham wire gauge is only used for iron and copper. The difference of density may be noted in comparison with Mr. Walker's table. When wire is drawn to five numbers any difference in density is eliminated. With reference to what Mr. Hall has said about gauges, the American screw gauge is a very beautiful instrument when well made, and those sold by Messrs. Churchill in Finsbury are of this character. I think with the screw gauge there is no difficulty, except the difference of a unit or two from the degree of force with which it is screwed down upon the wire; but the C.V.W. gauge may be read to the thousandth of an inch. I think the addition of weight for certain units would be a great advantage.

Professor ABEL: I can corroborate from experience what Mr. Hall has stated with regard to the extreme difficulty of relying upon any gauge as being applicable to the finer numbers of wires. I have had to do with wires of the thousandth of an inch, and I procured the best scale micrometer, but I have found wires up to a certain gauge—particularly those which have alloys of platinum or silver—differ materially in electrical conductivity, though of precisely the same composition; showing there must be some difference of gauge. I soon however adopted the plan of different weights for different lengths, and by that means I never had any difficulty in laying down specifications for very fine wires. No doubt in the case of wires of larger dimensions, the establishment of a gauge, as pointed out, would be of very great importance indeed.

Mr. LATIMER CLARK: On the subject of the screw gauge, I would allude to a little contrivance which was made by my partner, Mr. Herbert Taylor. He applied to the gauge a friction handle, by which the screw is prevented from going beyond a certain pressure, therefore in the most careless hands you get a uniform measurement.

Mr. WALKER in reply said: I should have felt it my duty to preface my paper with an apology to Mr. Latimer Clark for having taken up a subject specially his, had I not in conversation told him what I had in hand, when he expressed himself pleased to find that I was taking up this subject, and urged me to continue it. You will not fail to note that the figures which I have brought before you are not original, but simply reductions from figures placed in my hands in another form, by Messrs. Johnson and Nephew, and that, in the case of the fine wires below No. 42, where the other figures terminated, the diameters I have assigned to them are purely tentative. From the remarks that have fallen from the gentlemen who have spoken—especially from Professor Abel—it is obvious that a standard gauge would be very acceptable, and of great value. If I have done nothing more in this short paper of mine than re-opened the question and re-called attention to the unsatisfactory position in which wire-gauges rest, I shall be perfectly satisfied with the results, and repaid for the few hours I have devoted to putting this paper into form.

The PRESIDENT: In rising to move a vote of thanks to Mr. Walker for his very interesting paper, I wish to make only a few remarks. It is certainly interesting to learn how the Birmingham wire gauge has arisen; and it appears both from what Mr. Walker and Mr. Latimer Clark have so clearly put before us, that the Birmingham gauge is not founded upon any particular system. It has simply arisen to satisfy an immediate want, and I cannot help thinking that if this Society are to take up the subject of gauges, they ought to take it up as a problem to be solved upon its own merits, and not simply with a view to improve a gauge admittedly imperfect, that has been handed down to us from the time when what was required of the wire drawers was of a totally different nature to what it is now. The other gauges that have come into practical use, viz: the millimetre gauge and the decimalised inch or "mils" gauge, are based upon a regular increase of diameter, and are preferable, I consider, to the Birmingham gauge, by enabling us to verify the correctness of each number by its known diameters, representing a definite proportion of the millimetre or the inch. But it appears to me to be more correct to make each successive

number of the gauge represent an equal amount of sectional area, or (supposing the specific electrical resistance of the material to be the same in all cases) an equal increment of conductivity. Such a gauge would be extremely convenient to the electrician, because he would only have to multiply the specific resistance of a material into the number of the gauge, in order to determine the conductivity of the wire. But the mechanician also would be satisfied with the change because each step from one number to another would represent an equal increase of weight and of mechanical strength. I therefore consider that the suggestion brought before us of appointing a Committee to consider the question of devising a rational wire gauge is a good one, and if that Committee should prove successful we shall have more occasions than one to thank Mr. Walker and Mr. Latimer Clark for having brought this subject before us.

On the motion of the President a cordial vote of thanks was passed to Mr. Walker for his Paper.

The following Candidates were then balloted for and declared to be duly elected:—

FOREIGN MEMBERS:

A. Floyd Delafield.
L. Bigelow Jones.
George W. Atkins.

MEMBER:

Otto Henneberg.

ASSOCIATES:

Robert Herne.
Thomas W. Nunn.
J. Maclure.

The Meeting then adjourned.

The Sixty-eighth Ordinary Meeting was held on Wednesday the 8th day of May 1878, Mr. C. V. WALKER, F.R.S., V.P., in the Chair.

The CHAIRMAN: The first paper for this evening is one on "Sound in relation to the Telephone," by Dr. Clarence J Blake, who is now in the United States. Mr. Preece had undertaken to read the paper, being well acquainted with the subject of which it treats; but is unfortunately prevented from doing so on account of his being engaged at the Society of Arts in reading a paper upon the Phonograph. You will recollect, no doubt, that in the paper which Professor Bell read here some time ago on the Telephone he stated that in the course of his investigations of the mechanism of the human ear in connection with the inception of the Telephone, he consulted an eminent aurist in Boston, Dr. Clarence Blake, who is the author of the paper which the Secretary will now read.

SOUND IN RELATION TO THE TELEPHONE,

By Dr. CLARENCE J. BLAKE.

MR. PRESIDENT AND GENTLEMEN OF THE SOCIETY OF TELEGRAPH ENGINEERS:—

The paper which I have the honor of presenting touches but superficially on a field newly opened for investigation. Though barely six months have elapsed since Professor Bell first addressed your Society, hundreds of active brains and dexterous hands are already cultivating the newly acquired territory with daily increasing prospect of an abundant harvest.

It is safe to say that within the last quarter of a century there has been no single scientific discovery which has proved so great a stimulus to investigation as that of the telephone, and that the gain to science from the solution of the problems which this instrument suggests will outweigh even the practical benefits from its use which are now a matter of daily experience.

The word "sound" naturally suggests for consideration that organ of special sense designed to transmit the mode of motion to the perception of which we give this name. The most delicate mechanism for the reception and transmission of a wide range of sonorous vibrations, of which we have any knowledge, is to be found in the human ear, and the portion of this intricate organ which has especial interest in connection with the telephone includes the membrana tympani or drum membrane and two of the three small bones of the middle ear with their accompanying ligaments. As this important portion of the sound-transmitting apparatus of the ear has already been availed of in the earlier experiments of Professor Bell, and as a consideration of the principles upon which it acts may offer further suggestions for the improvement of the mechanism of the telephone, and as furthermore it may, when properly prepared, be used for the graphic illustration of the sound waves which it transmits, a part of the time at our disposal may justly be devoted to a consideration of its structure.

The organ of hearing in man is divisible into three parts,—the external, the middle, and the internal ear.

The latter division includes within a cavity in the petrous bone the terminal fibres of the auditory nerve, and an exceedingly delicate apparatus for the final transmission to these fibres of the sound-waves received through the external and middle ears.

The first division includes the outer ear, or auricle, and the external auditory canal, a passage about an inch and a quarter in length, leading inwards to the drum membrane. The drum membrane, which forms the boundary between the external and middle ear, is from eight to ten millimetres in diameter, and is inclined with the plane of its surface at an angle of about forty-five degrees to the long axis of the external auditory canal. The centre of the membrane being depressed, its outer surface presents the form of a funnel, the sides of which are convex outwards, from the centre to the periphery of the membrane.

Beyond this membrane lies the middle ear, an irregular, bony chamber, from three to fifteen millimetres in diameter in its several parts. This cavity is in communication with the pharynx, through a narrow canal, the Eustachian tube. One important

office of this tube is to insure the ventilation of the middle ear, and to provide for the equal degree of atmospheric pressure on both sides of the drum membrane, necessary to the proper vibration of that structure.

Fig. 1 exhibits a vertical section through the head, showing the relations of the external auditory canal, the drum membrane, middle ear, Eustachian tube, and pharynx.

Within the cavity of the middle ear are placed the three small bones which convey the vibrations of the drum membrane to the internal ear.

Fig. 2 exhibits a vertical section of the three divisions of the ear, showing the inner end of the external auditory canal, the drum membrane, and the cavity of the middle ear with the three small bones. 1. The malleus attached to the drum membrane along its lower half; 2, the incus articulating with the head of the malleus; and 3, the stapes attached to the lower end of the incus, and fitting into an opening of the bony wall of the middle ear cavity, the "oval window," communicating with the cavity of internal ear.

Crossing the cavity of the middle ear at its upper part, and attached to the malleus by its tendon is seen the tensor tympani muscle, the contraction of which increases the tension of the drum membrane, and of the ligaments connecting the small bones.

The sound waves entering the external auditory canal beat upon the outer surface of the drum membrane, and set it in corresponding vibration.

The peculiar curve of the membrane and the difference of tension in its several parts enable it to respond readily to the impact of a wide range of musical tones, extending in the normal ear from a tone of thirty-two vibrations to one of over 40,000 vibrations in the second.

Under contraction of the tensor tympani muscle, the limit of transmission is increased by about 5,000 vibrations in the second. Beyond this pitch the membrane seems to present an obstacle to the passage of sonorous vibrations, as measured by the real limit of perception for high musical tones, which is much above that of the transmitting capacity of the membrane, as I have been able, in cases of perforation with an otherwise normal ear, to determine,

by means of König's rods, a perception of tones as high as 80,000 vibrations in the second.

The extreme delicacy of the perceptive apparatus of the internal ear capable of conveying to the brain the impulse of a sound-wave, of which 40,000 in a second strike upon the membrana tympani, is illustrated by the slight excursions of this membrane under the impulse of much lower tones, the movement of the centre of the membrane in response to a tone of 630 vibrations in the second being only about eight one thousandths of a millimetre.*

In preparing the ear for use as a phonautograph, the roof of the cavity of the middle ear is first cut away; through this opening a narrow-bladed knife may be introduced to divide the tendon of the tensor tympani muscle and the articulation of the incus with the stapes. By means of a hair-saw a section of the middle ear is then made from before, backward through the divided articulation. This section removes the inner wall of the middle ear cavity with the portion of the bone containing the internal ear and exposes (Fig. III.) the inner surface of the drum membrane, with the malleus and incus attached. [In this figure the stapes is also retained, in order to show its relation to the other small bones.]

An important point in the mechanism of vibration of the drum membrane, malleus, and incus to which I would draw your attention, is the fact that the greater portion of the two bones projects above the periphery of the membrane, and that under the impulse communicated to them from the membrane they swing, when acting in concert, upon an axis very nearly represented by the dark line drawn across the plate. This axial line terminates at one end by a ligament attaching the malleus to the bony wall of the middle ear cavity, and at the other end by a ligament similarly attaching the point of the incus. The two bones are held together by a third, a capsular ligament, which invests their articulation.

In the spring of 1874, whilst studying the vibrations of the membrana tympani under tension, I was led to measure the distribution of weight in these bones above and below the axial

* C. H. Burnett, Philadelphia. Measurements of Vibration of the Membrana Tympani and Ossicles. 1874

line, with a view to determining any mechanical value which this distribution of weight might have as a counterbalance.

The results gave an average preponderance of weight above the axial line, over that below, in the proportion of fifteen to eight. When the tensor tympani muscle contracts, the bones are pulled away from the wall of the middle ear cavity, the drum membrane and the ligaments of the bones are rendered tense, and the counterbalance enables the whole vibrating apparatus to respond more readily to a slight impulse. In using a preparation of the ear as a phonautograph, a stylus made of a single fibre of wheat-straw is glued to the descending part of one of the small bones, parallel to the long axis of the bone. With this, tracings may be made upon a plate of smoked glass, sliding upon a glass bed at a right angle to the line of excursion of the drum membrane, and moved by clock-work or a falling weight, as in the apparatus mentioned by Prof. Bell in his communication.

Figs. IV., V., VI., VII. and VIII. represent tracings of vowel sounds made in this manner, the upper line being a tracing of the vowel sung at the pitch marked upon the plate, the lower line, the same vowel sung one octave higher, illustrating the effect of tension upon the excursion of the membrane.

In using a preparation of the ear as a telephone, the adjustment of the counterbalance may be still further availed of, by cutting away all attachments of the small bones with exception of the three principal ligaments already mentioned. With an ear so prepared, having a disc of ferrotype plate, seven millimetres in diameter, glued to the descending portions of both the malleus and incus, and with a proprtionately small magnet and coil (resistance 44 ohms), I have been able to carry on conversation without difficulty over a line something more than six hundred feet in length, the ear telephone being used only as a receiving instrument.

The stiffness of the preparation from accidental drying and the proportionately great weight of the disc, made it impossible to detect more than a single tone communicated from one ear to the other, but with a more fortunate preparation and better adjustment of the discs and magnets, better and possibly valuable results may be hoped for.

The mouth-piece of the hand telephone may be compared to the external ear, the metal disc to the drum membrane, the air-chamber to the middle-ear cavity, the damping effect of the magnet to the traction of the tensor tympani muscle, and the induced current in the coil to the sentient apparatus. Beyond these grosser comparisons the analogy ceases. The principal coat of the drum membrane of the ear is composed of two layers of fibrous tissue; the fibres of the outer layer are arranged in a radiating, those of the inner layer in a circular direction. No single fibres extend either from the centre to the periphery or follow the periphery of the membrane, but cross and interlace, maintaining only these general directions.

The result is a structure possessed of great elasticity and strength; the same membrane which will transmit a complex sound-wave of a high tone will bear the pressure of a column of mercury equalling its own diameter, and over two centimetres high, without injury.

The artificial tissue which, perhaps, best represents this fibrous arrangement is found in the felting together of the fibres of some qualities of paper; and the use of paper discs in receiving telephones, by reproducing more accurately the slighter overtones, and cutting down, as it were, the more pronounced overtones, to which especial prominence is given by the metallic disc, renders the voice, though somewhat lessened in intensity, with a more natural and pleasing modulation.

As the limit of perception in the ear exceeds the limit of transmission of the drum membrane, so the limit of what may be called the carrying power of the current induced far exceeds the transmitting capacity of the metal disc, the character of the disc determining the limit of transmission. The following tests with König's rods, made with hard rubber case hand telephones connected by an overhead wire about 600 ft. long, may serve to illustrate this. With the discs of both telephones of ferrotype plate, a tuning-fork, 1,024 vibrations in the second, held over the mouth-piece, was heard faintly. The mouth-piece being removed, and a König's rod suspended in front of the plate at a distance of three millimetres, a tone of 8,192 vibrations in the second was heard plainly, and a

tone of 10,240 v. s. but very faintly; with a tone of 12,288 v. s. only a faint tap, but no musical tone, was heard. The iron disc was then removed from the transmitting telephone, and the König's rods suspended so that they nearly rested on the flange of a rubber case at a distance of 0·8 millimetres from the magnet.

Under these conditions a tone of 10,240 v. s. was heard plainly and a tone of 12,288 v. s. but very faintly. Above this tone only a tap was heard.

The metal disc was then also removed from the receiving telephone, and a disc of postal-card paper, having a disc of ferrotype plate fifteen millimetres in diameter, fastened to the centre, was substituted, the König's rods being suspended in front of the magnet of the transmitting telephone, as before.

The increase in the limit of transmission of high tones was very decided, tones of 12,288 v. s., 16,384 v. s., 20,000 v. s., and 25,000 v. s., being plainly heard, and a tone of 30,000 v. s. faintly but distinctly perceived. Beyond this tone only the tap of the striking hammer could be heard. The person listening in this case had a normal ear, and could easily hear a tone of 40,000 v. s. when the rod struck was suspended three inches from the ear. The rods were set in vibration by a steel hammer striking with uniform force. With some of the telephone plates tested, the limits above mentioned could not be reached, a slight indentation of the plate, or an inequality in thickness, considerably modifying its vibration.

The manner in which different telephone plates will reinforce those tones to which they vibrate sympathetically, is easily illustrated by connecting several receiving telephones with a transmitting telephone, into which a series of organ pipes or the notes of a cornet are sounded, slight differences in the plates and in the damping effects of the magnets, giving prominence to certain tones.

In studying the relations of sound to the telephone, the subject of the transmission of the human voice especially commands our interest; and since it is in this direction that the greatest practical benefit from the telephone has as yet been derived, I have made use of the human voice, in preference to a musical instrument, for illustration in the tests to be described.

The sound chosen was that of the broad vowel sound *a*, at a

pitch of 488 v. s., the repetition of the tone being insured as accurately as possible by comparison with a tuning-fork, giving a fundamental note of that pitch. The mechanical vibrations of the discs of both transmitting and receiving telephones, or more accurately the excursion of the centres of the discs, was measured in two ways, first by attaching a platinum wire to the centre of the disc, the end of the wire being coiled in a gradually widening spiral the base of which was glued to the centre of the disc, ten centimetres in diameter, of a box telephone. The free end of the wire was sharpened to a point 0·01 millimetre in diameter, and bent downward at a right angle to come in contact with a plate of smoked glass, moving horizontally, at a right angle to the line of excursion of the disc, on a glass bed supported by brackets firmly screwed to the wood-work of the telephone.

The two telephones thus prepared were clamped in an upright position, and connected by flexible wires. On singing into the mouth-piece of the telephone the platinum point recorded the excursion of the disc upon the smoked glass, a slight movement of which presented a fresh surface for tracing. A large number of tracings could thus be made upon one plate with the least danger of disturbing the adjustment of the stylus. Platinum was chosen for the wire on account of its ductility, and the arrangement of the coil at its base was made to insure as little individual motion as possible.

The excursion of the transmitting telephone disc, without the magnet, was first recorded, the average of all the readings for the vowel *a*, 448 v. s., measured under the microscope with micrometer eye-piece, being 0·2625 millimetre. The magnet being re-adjusted, the deflection of the centre of the disc due to magnetic attraction was found to be 0·061 millimetre. As would be expected, a deflection of the plate to this extent would produce a very appreciable difference in its excursion, and the average of the readings with the same tone under traction of the magnet was found to be only 0·190 millimetre, a difference in the length of the excursion of 0·0725 millimetre, or about 27·65 per cent.

In listening at the telephone we are conscious of a considerable loss in the volume of the sound; the loss is really greater than we

might be led to expect from the evidence of our hearing, since the ear accommodates itself to the reception of tones of slight intensity by the contraction of the tensor tympani muscle, thus compensating, in a measure, by an increase in its own power of transmission and perception.

The movement of the receiving telephone disc as recorded on the smoked glass was so slight that a much larger number of tracings was made than with the transmitting disc, in order to allow for any accidental disturbances; the average of these different sets of tracings, however, was always within 0·02 millimetre. Controlling tests were then made with a micrometer screw connected with a delicate galvanometer, the results gained being much more uniform, and confirming the previous measurements of the transmitting disc, while the average excursion of the receiving disc was determined at 0·0135 millimetre, a loss in motion of 92·9 per cent. between the two telephone discs.

In order to further exhibit the difference in the excursions of the discs, and test the vibrations in the air cavities beneath them, the cups of two hard rubber hand telephones were tapped, and short tubes inserted, making connection with two manometric flames, the telephones were connected by flexible wires, and the vowel sounds sung into one of the telephones, with the results given in Figs. IX, X, XI, XII, representing respectively the vowel sounds *o*, *a*, *e*, and *u*, the upper line being the flame given by the transmitting, and the lower line by the receiving telephone.

The size and form of the air-chamber also exerts an influence upon the vibration of the telephone disc; if the chamber is tightly sealed, the excursion of the disc is diminished by the resistance of the compressed air beneath it, and the clearness of the tone transmitted, may be increased by making an opening into the chamber, which may be compared to the opening of the Eustachian tube into the middle ear, as has been done by Mr. Watson.

Another important element in the production of articulate speech, which has its influence upon the movements of the telephone disc and upon the induced current, is the force required for the production of the consonant sounds.

Since a certain definite position of the articulating apparatus is required to produce each consonant sound, it would be expected that the distribution of pneumatic pressure would be more or less constant for each consonant pronounced independently. In a paper read before the Royal Society, April 16, 1874, Mr. Barlow describes an ingenious and valuable instrument constructed for the purpose of measuring the degree and distribution of the pneumatic pressure resulting from the production of articulated sounds.

The diagrams accompanying the paper show the degree of precision with which the instrument fulfils the object for which it was designed, and show moreover a curve more or less characteristic for each consonant. The degree and distribution of pressure thus indicated may, for convenience, be termed the logographic value of the consonant; as is further shown, the logographic value of any one sound is modified by combination with other sounds as in spoken words or sentences, so that in speaking against a membrane, a telephone disc for instance, we have not only the shorter excursions of the disc which correspond to the musical value of the tone, but also the larger excursion in response to the pneumatic pressure. That this larger excursion, by changing the tension of the plate, should exert an influence upon its vibration, and so upon the second telephone disc through the effect upon the induced current, is shown by tracings of consonant sounds, as exhibited in—

Fig. XIII. Excursion and logographic curve of consonant sound D.
„ XIV. Logographic curve of D, with vowel sound expressed.
„ XV. Excursion and logographic curve of B.
„ XVI. Logographic curve of B, with vowel sound expressed.
„ XVII. Excursion and logographic curve of F.
„ XVIII. Logographic curve of F, with vowel sound expressed.

These tracings made with the human drum membrane are an attempt to exhibit—1. The excursion of the membrane under the

pressure employed in producing the consonant, the recording plate being fixed: 2. The logographic curve of the consonant, the recording plate moving at the rate of five millimetres in the second: and 3. The logographic curve with the sound-waves, the recording plate moving at a rate of about five centimetres in the second.

The approximation of the telephone disc to the magnet under this pressure increases also the damping effect of the latter upon the disc, which may in a measure account for the greater difficulty in transmitting intelligibly by telephone consonant sounds having the highest logographic value, that is, those which would produce the greatest excursion of the disc, such as the explodents T, P, D.

The extent and duration of the depression of the transmitting telephone disc may also be measured through the effect of the produced current upon the galvanometer.

A hard rubber case hand telephone (resistance seventy-five ohms) was connected by about forty feet of copper wire (resistance 0·342 ohm) with a Thomson short coil reflecting galvanometer (resistance 0·645 ohm), and the consonants sounded into the telephone with the mouth enclosed in the cup of the mouth-piece.

T, the consonant sound of greatest logographic value, gave the greatest deflection, and was therefore taken as a standard. The following table gives the percentages of the measurements with the galvanometer:—

T, 100.	C, 62.
B, 53.	S, 40.
P, 58.	F, 35.
D, 45.	V, 62.
K, 31.	R, 19.
G, 56.	Z, 53.
L, 21.	
M, 9.	
N, 11.	

representing fairly, but not accurately, the logographic value of the consonant sounds; the excursion with M and N being noticeably small, on account of the direction of pressure, not outward through the mouth, but backward in the naso-pharyngeal space and nasal cavity.

A few tests made with a Thomson quadrant electrometer (resistance 6 ohms) served to confirm very nearly the record above given, and, in addition, a perceptible broadening of the light spot thrown upon the scale was noticeable, with the accompanying vowel sound of the consonant. On sounding the consonant an octave higher, this broadening was nearly doubled.

When we consider the complex character of the waves resulting from the production of articulated sounds, and the loss in the excursions of the receiving disc, the wonder grows that this piece of metal can by its mechanical vibration reproduce so clearly and distinctly the delicate shades of quality of the human voice.

That this should have been so perfectly accomplished is the result not of inspiration, but of laborious research, and the instrument of which we reap the benefit to-day is the product, not merely of the genius, but of the patient and persistent labour of Alexander Graham Bell.

Major Webber, R.E.: Mr. Preece should have been here to ask you to return thanks to Dr. Clarence Blake for the very interesting paper which he has communicated to the Society at Mr. Preece's request. We can none of us forget that it was in connection with the instruction of the deaf that Professor Graham Bell was first struck with the idea of producing sound by means of the telephone; and here we have to-night, in the diagrams which have been given to us, the ear used as an illustration of the experiments which Dr. Blake has made with that instrument. Although some portions of what we have heard would probably have been more interesting to, and better understood by such of our members as belong to the surgical profession; and although the results which have been obtained by Dr. Clarence Blake may be made use of by them, yet I think this Society generally is interested in them, because we find that he has been able to make in such experiments an application of the telephone in a way which we could scarcely have realised. I ask you, then, to let our thanks be awarded and communicated to Dr. Blake for the very interesting paper with which he has so kindly favoured us.

Lieut.-Colonel Bolton: I have much pleasure in seconding the proposition made by Major Webber, and I may take this

opportunity of informing the meeting that it is intended to have a special telephonic night towards the end of this month, if it can be so arranged without inconvenience to the Institution of Civil Engineers, to whose kindness we are indebted for the use of this room. On that occasion Dr. Blake's paper will be discussed, together with other matters of great interest connected with the telephone.

The vote of thanks to Dr. Blake was unanimously and very heartily carried.

The CHAIRMAN: I have now the pleasure to introduce to the Society Mr. Frederic A. Gower, who will read a paper on the same subject, under the title of "The Telephone Harp," and the instrument to be shown is of his invention.

THE TELEPHONE HARP.

Mr. GOWER said: Mr. President and Gentlemen of the Society of Telegraph Engineers: It seems especially fortunate or especially well advised, that the production of distinctly audible telephonic sound, which I hope to accomplish this evening, should have been assigned to the same occasion as the more learned and interesting paper on a kindred subject to which we have been listening. And I am glad to have the opportunity of saying, for the benefit of any who may not have heard it before, that Dr. Blake has been among the most earnest and successful investigators of the speaking telephone and its laws, at a time when such investigations promised little of profit or distinction. It is largely to his intelligence and zeal that the telephone owes its early growth, and I regret that he is not able to be with us this evening to examine some of its later developments.

Since we have had, in the paper just read, a discussion of some of the more intricate physical and physiological principles involved in the action of the telephone, it seems the more desirable that I should employ a part of the limited time which I venture to use, in a brief survey of the progress of the telephone, and the extent of its practical use, as well as in the exhibition of the special form of instrument which I have to bring before you.

If, as it has been said, the year 1877 is to be remembered as the

year of the articulating telephone, so may the telephone itself be said to mark the era of popular interest in scientific *methods* as distinguished from ready acquiescence in the practical results. From the time when, in the spring of 1876, the researches of Professor Bell gave the word telephone a place in current speech, and linked it with a fact accomplished, there has been an active interest among all civilized peoples, not only in the instrument itself, but also in the abstruse scientific reasoning upon which the laws of the instrument stand. The newspaper press in the United States gave itself over to scientific research without delay; the professedly scientific journals followed. At the Philadelphia Exposition, in the summer of 1876, Sir William Thomson set upon the new wonder the stamp of accepted scientific judgment, and then its fame, as you remember, ran about the world. It supplied imagery to the Press, a mild inspiration to the poet, and suggested to the progressive pulpit a time "when the sea should be reft by the lightest whisper, and the earth be belted by one spoken word," a state of things towards which we are still upon the way. In the winter of 1876 a number of dual lectures were given, Professor Bell and myself addressing an audience at either end of the line, while Mr. Thomas A. Watson transmitted telephonic effects to both audiences simultaneously from a midway station. The total length of wire used at these times varied from thirty to sixty miles, and though the sounds produced were not loud, the fact that sound could thus be produced at all, gave stimulus to the public interest in the telephone. As a relic of these earlier lectures, there is before us this evening one of the identical instruments used in them, and which was, as I believe, one of the very first instruments employed to convey the human voice along a wire. It stands precisely as it was made, is available as a speaking telephone, and embodies the principle contained to-day in the speaking telephone throughout the world.

Few of these dual lectures had been given before the American public began to draw the inference that if speech could thus be *faintly* sent from city to city it might be *loudly* sent from house to house, and thus the telephone be made available for every-day communications. A demand for practicable telephones sprang up, and telephonic experiment tended more sharply towards utility. The

instruments used for public lectures were comparatively loud, but not distinct; and this has been the bane of all loud telephones so far as I am aware. But it was soon learned that distinctness could be gained at the expense of volume of sound, and at length, through investigations undertaken for Professor Bell by Mr. John Pierce, Professor Eli. W. Blake, and Dr. Wm. F. Channing, at Providence, Rhode Island, as well as the unceasing efforts of Mr. Thomas A. Watson, the resonant box was quite done away with, and the telephone reduced to the convenient and effective form in which you see it now beside the original lecture instrument—these two types exhibiting the telephonic evolution.

The practical success of these "hand telephones" was beyond question, and this, in turn, led Mr. Watson to a modified form of the box telephone, in which a horse-shoe or double-pole magnet is used. These two classes now form the majority of telephones used in the United States, and the total number in use at the end of the first year (June 1, 1878,) will exceed 20,000, most of them under licence from Professor Bell. As to the forms of use of this mass of instruments, it may be said that they are employed upon lines ranging in length from thirty or forty feet to as many miles, and for almost every sort of public or private communication—from the intercourse of the White House and the Treasury Department at Washington, to the exchange of village gossip in the remoter sections of New England. A few cases may, perhaps, not improperly be cited. In the seaside city of Newport, Rhode Island, there is an over-house line having forty-six stations, with a single wire circuit carried to earth at each end. At each house or station is a Morse "sounder" and a telephone; and signal having been made upon the sounder by a battery current supplied from the main office of the line, the telephone is brought into circuit and conversation carried on. Similar systems, though with usually a less number of stations, are found in other large towns. A more common form, however, is the central-office system, in which lines radiate from a central station, ten to twenty houses or shops being joined together upon each line. It is not found that this union of different interests upon the same wire is practically an obstacle to the development of the system, unless the number of stations on

one circuit comes to exceed the working capacity of the line, in which case a new circuit is begun. In perhaps the greater number of these town and city systems, no batteries are used, the manufacture of magneto-electric bells, under Mr. Watson's patents, having produced a satisfactory quality in these instruments. Nor is this use of the telephone restricted to comparatively quiet situations. The instrument has been found to make itself quite at home under the conditions usual in manufacture and trade. The central-office system at Boston included, at the time of my departure in March, 256 telephones placed in shops and offices in the busiest parts of the city, besides those in the residence and suburban quarters. I have seen these instruments at work in railway managers' offices, and the counting-rooms of factories, where the noise was so great as to lead the visitor speedily to escape to the open air. The clerks, in these cases, being questioned, usually answered that practice enabled them to disregard the local noises, while the holding a telephone at each ear enabled them to listen with the greater effect. It is, however, but just to say that the average quality of telephones supplied in the United States, has, until lately, been superior to that at command of the public in England. But I observe that instruments are now making in London nearly, if not quite, equal in power and distinctness to those made elsewhere, and I am therefore led to believe that at no distant day the commercial importance of the telephone will be as well recognized in England, which gave the inventor, as in the United States, which fostered the invention.

But I am conscious of straying too far from the special topic assigned—the Telephone Harp. This instrument originated from the need of some means of making telephonic effects more clearly audible for large assemblies. Sound, and occasionally something like the words of a song, could be heard at a distance from the receiving telephone, but this did not satisfy the public curiosity. Something new was evidently needed, and therefore I began, at the time of Professor Bell's departure for England in August, 1877, the attempt to produce a transmitting instrument which should enable the box telephones which we had been using to produce musical notes audible in any room. The Mason and Hamlin

Organ Company of Boston gave me every facility for experimenting at their factory, and Mr. M. J. Matthews, a member of that Company, contributed invaluable assistance in musical and mechanical skill; and thus, after six months' experiment I was enabled to produce an instrument which seems, in some measure, to meet the demands of public telephone exhibitions, and which, in its audible effects, I hope now to bring before you.

The arrangement for bringing the sound-producing currents into the room is so simple as hardly to need description. From the transmitting instrument, placed in the small room behind the platform, the wire comes to my table, passes through the telephones here, is continued through the two telephones in the farther corners of the room, and returns as it entered, giving us a metallic circuit, with the privilege of using one, two, or four telephones at once, as we may chose. In the transmitting room is placed the Telephone Harp, with a battery of four Leclanché cells, and a Ruhmkoff coil giving a spark of $\frac{1}{32}$th of an inch in air—a coil of the insignificant sort often supplied with electro-medical apparatus. The length of the circuit is a matter of very little consequence, and I have not thought it necessary upon this occasion to remove the transmitting instrument farther than merely out of our hearing, through the air. To communicate with the transmitting room I have the telephones, as you see them, and I will ask you to regard their use as merely incidental to the experiments, quite as one might address oneself to assistants visible upon the platform. As my chief assistant, I have the honour to present to you Mr. Thomas Fletcher, a member of this Society, and connected as an electrician with the Telephone Company. I will now ask the musician to begin playing. [Audible music proceeded from the telephones about the room.] You perceive the power which is latent in the ordinary telephone, and which is yet, I think, to be more generally applied. I do not, as a matter of course, regard this music as excellent in quality, nor am I unaware of means by which its quality might be improved. But I have chosen to receive and diffuse this effect through the ordinary telephones which can be used for conveying articulate speech, rather to use receiving instruments which, though giving us smoother and more melodious sounds,

would have no especial significance as speaking telephones. But being now provided with a clearly audible telephonic sound, we are enabled to attempt a variety of experiments with telephonic vibrations, to a few of which I will invite your attention.

As the transmitting instrument is so near, we will first ascertain what sound, if any, reaches us through the air. The music again being heard, I shorten the circuit to exclude the telephones, and hear no sound. Restoring the longer circuit we hear the musician going on as before. Being thus assured that the whole volume of sound comes to us through the telephones, we may naturally examine next the extent and kind of power developed to form these sounds in the telephonic apparatus. Referring especially to the large—or, as it may be called, the original—telephone, you observe that it consists of a magnet with coils, and a vibrating diaphragm, in the usual form, except that the magnet is mounted upon a moving bed controlled by a thumb-screw by which the position of the magnet may be changed.

Confining the circuit to this instrument alone, I draw back the magnet, increasing the distance between the poles and the diaphragm, the loudness of the musical sound falls off in proportion. There is now a clear half-inch between the poles and the diaphragm, yet the sympathy is not so weakened as to prevent our hearing the sound. I place the palm of my hand full against the diaphragm upon the one side, and the tips of three fingers against it upon the other, yet I cannot dampen the extraordinary persistence of this vibration. Removing the magnet still further, there is now a clear inch of space between the magnet and the diaphragm, yet the sound is still audible, though faintly, to the President and Gentlemen in the immediate vicinity of the instrument. Restoring the magnet to its usual place, the sound regains its volume, and increases up to the moment of contact between magnet and diaphgram, and then it suffers loss both in quantity and quality. But still there is a sound, and that, although I have now sent the magnet against one side of the diaphgram with the whole power of the screw, and also placed the tips of three fingers firmly against the other side. It is, as you see, quite impossible to suppress this vibration without either breaking the electric circuit or destroying

the instrument. I think it may be said that this persistence of sound producing vibrations is exceptional at the least, and seems to give colour to the theory of molecular vibration. Yet I hope to show within a few moments that these same sounds are attended with a distinct movement of the diaphragm from the centre, so that we may secure an amplitude sufficient to open and close a galvanic circuit by the vibration of the diapragm in a receiving telephone.

It is of interest, perhaps, in passing, to note that the intensity of these sounds can be reproduced from even a small hand telephone intended only for practical speaking purposes. Shortening the circuit, I concentrate the effect upon the ebonite telephone which I have been using to speak through to the transmitting room, and you perceive that the notes are still easily heard throughout the hall. The quality, of course, suffers, since this instrument is wholly without a resonance cavity, but I think that the quantity may be said to be quite beyond question.

Of the transmitting instrument, I can say only that it consists in a series of contact-breakers working with delicacy sufficient to open and close the circuit at such rate as the natural vibration of each note may require. In outline it consists of a harp of steel tongues struck by pianoforte hammers and action from beneath, and made to vibrate through supplemental tongues against the points of contact screws. The duration of each note, as heard in the receiving telephone, is controlled by an automatic circuit-closer, worked by the musician's touch upon the key, and in this way confusion of notes is avoided, even in rapid movements like that from the overture to "William Tell," which you have heard. Various details in the construction may be of interest, and therefore the harp itself will be brought visibly before you at the close of the experiments.

Having thus obtained clearly audible sounds which did not, at the least, offend the ear, it occurred to me, in January of the present year, that these sounds might also be rendered visible in the form of their equivalent electric currents. With the aid of Mr. Watson, whose ingenuity is unfailing, this idea was put into practicable form, and I shall endeavour to show it as my last experiment this evening. In this, we proceed upon the theory that the vibration of the diaphragm is membranous—from the centre in

and out --just as, in the attempt to dampen the vibration we seemed to show that it could only be molecular. In circuit with the other receiving telephones, and only far enough away to avoid offending our ears with its jangling, is a form of box telephone, in which a double-pole magnet and coils impart to an iron disc of the usual form such *amplitude* of vibration that the movement of its central part opens and closes a galvanic circuit, of which the whole disc forms a portion. This circuit is joined to the primary wire of a Ruhmkoff coil, which you see upon the table, and whose secondary circuit is made through a vacuum tube.

We will now have the room darkened, the musician will begin to play upon the harp, and you will both hear and see the effect. The currents passing through the telephones produce audible sounds. The same currents, considered with respect to the relay, or interrupting telephone, produce also the rhythmic interruptions in an auxiliary circuit which the vacuum tube converts into light. The effect in the tubes is not strictly constant to the audible sound, because the consumption of the platinum at the points of contact makes it well-nigh impossible to maintain that close adjustment which so very slight an interruption of the primary circuit requires, but the response is sufficiently accurate to show, as I think, that the telephonic disc has a distinctly membranous vibration.

What office this vibration performs in the production of sound, or how it persists—if persist it does—in spite of attempts to check it by mechanical pressure, are questions which have for me an absorbing interest, but upon which I must confess to the possession of very little accurate knowledge.

I have now, in closing, only to thank you for the kind attention and the generous applause which you have given to these experiments, and to say that it will give me pleasure to explain any part of the apparatus used to gentlemen who may wish to make personal inspection.

A cordial vote of thanks was passed to Mr. Gower on the motion of the Chairman.

The following Candidate was balloted for and declared to be duly elected—

MEMBER: John Perry.

The Meeting then adjourned.

At a Special General Meeting, held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, May 23rd, 1878,

C. W. SIEMENS, F.R.S., D.C.L., President, in the Chair.

The PRESIDENT: The first communication which I will call upon the Secretary to read is one from Lieutenant Savage, upon some experiments regarding the telephone, which will form a good introduction to the chief communication of the evening by Mr. William Preece.

The SECRETARY read Lieutenant Savage's communication as follows:—

FIELD TELEGRAPH OFFICE, ROORKEE,
7th March, 1878.

I have been making splendid big telephones lately, and they will do anything—almost talk of themselves. I have large ones now that are almost too heavy to lift, and they *do* speak out; they make nothing of very long distances.

I have gone regularly to work, and I find the *sending* telephone is of the two the one to aim at. I tried improving the *receiving* telephone, but there is no very wide field for improvement in it. It is impossible to increase a current of electricity without expending energy on it in some way, and it seems impossible to apply energy in any shape to increase the current as it arrives at the receiver. So in the receiving telephone it is only necessary to take the current as it comes, and get the most possible work out of it by having a highly-magnetized magnet, a coil of tolerably high resistance in comparison with the total resistance in circuit, and a thin (very thin) vibrating disc, so as to oppose the work as little as possible. But in the *sending* telephone I have found the greatest advantage by having large ones—large strong magnets, 18″ long and ½″ section (diameter); coils of very high resistance placed advantageously (some of my coils are 2,000 ohms, when working on very long lines), and a large thin vibrating disc 6″ across, and a large open mouthpiece 2½″ across. One I have just

completed of this size is quite a startler; even at long distances it seems as if the sender was shouting in one's ear. I am going to try about 400 miles with this one to-morrow. I have had some telephones supplied to me by the Government (Telegraph Department), but they are of no use at all. I can hardly hear a faint sound, as of a fly humming, at a few miles, so I have not been able to get a wrinkle from them. I should like very much to send you one of my big telephones to test beside others in England. I made the current from a telephone induce a current in another separate coil, this separate coil being circuited through a receiving telephone as follows: I wound about 300 ohms of fine wire on an iron core, and on the top of this I wound about 300 more ohms, the former coil was circuited through the sender (70 miles off), the latter separate coil was on to my receiving telephone. I heard quite distinctly with the induced current, but of course *no better*, as there was no *energy* expended to increase the current; it was a pretty experiment, that was all. I fitted a vibrating disc above the end of the iron core, and magnetized the iron core; the coil became a telephone, and as work was done in vibrating this last disc I could hardly hear anything on the regular telephone, it was rather a neat way of changing (1) sound into vibrations, (2) vibrations into electricity, (3) electricity into magnetism, (4) magnetism into induced current, (5) induced current into magnetism, (6) magnetism into vibrations, and sound.

The limit of the sound that can be produced by a telephone is the amount of energy which can be expended by the voice on the sender's telephone. I mean that it is advantageous to expend a considerable amount of energy on the sender's vibrating disc, and to convert as much of this energy into electricity as possible—voice energy is the source of the electricity, and I think the limit of the voice energy must be the limit of the electricity—converting the voice energy into as much electricity as possible by proper apparatus.

Voltaic electricity can be increased by increasing the number of cells, but the voice has its limit.

I do not think that the current sent by a powerful telephone is so small, so very small. I think the reason it does not affect a sen-

sitive galvanometer is because it is reversed so many hundred times or thousand times a second that it never has *time* to operate on the galvanometer needle.

Somehow I do not *think* that the limit of telephones *for speaking* will be found to be in the amount of electricity generated; but it will be found first in the fact that this kind of electricity seems to travel slower at the receiving end of a *long* line than at the originating end, hence of course another sound is commenced before the previous one is completed: there is a jumble, and of course clang tones of talking cannot be distinctly heard. I use large plates to get amplitude of vibration—the greater the amplitude of disc vibrations (so long as correct tones are emitted) the greater the quantity of electricity; also the loudness of the note sounded by the sender is in proportion with the square of the amplitude of disc vibration, and the greater the mass of the vibrating disc the more current is originated.

I think that the resistance of the coils of the sending telephone ought to be about equal to the total resistance of the circuit (exclusive of other telephones).

Has anyone satisfactorily proved that it is the approach and recession of an iron disc to a magnet? Would you be very much surprised to hear that it was the vibrating particles of the magnet —while the coil does not vibrate—so the lines of force which are permanently attached to each atom are cut by the coil when they vibrate? I thought this two months ago. I wonder is there anything in it? That the particles of the bar magnet vibrate longitudinally. I don't think there is much in this, but it seems strange that the following should happen. I take the vibrating disc off a telephone, and I tap with a thick bit of brass on the end of the exposed magnet—this tapping I can hear on a distant telephone. Is it only because brass may be called in some degree a magnetic substance, though in a small degree, or have particles (magnetic) vibrating got anything to say to it? I have also been able to hear talking when using a brass vibrating disc instead of iron—though of course very much more faintly—the explanation is that brass is more or less a magnetic substance (principally less).

But it would seem that although Professor Bell's explanation of

his telephone is evidently correct, still you can transmit sounds by making the magnet or coil vibrate relatively to each other, if you take off the iron vibrating-plate of a telephone and connect it in circuit with another telephone; if you tap any portion of the telephone which has no disc, the wood, cane, coil, magnet, with anything you like, you can hear that tap transmitted to the other telephone. I suppose the tapping disturbs the relative positions of coil and magnet, and so the lines of force of magnet are cut by the coil, hence a current which is transmitted to the other telephone. So why cannot one make a telephone on this principle also—a vibrating magnet—and a permanent fixed coil, or *vice versâ*, and no iron disc?

The PRESIDENT: I call upon you, Gentlemen, to return a vote of thanks to Lieut. Savage for his very interesting communication.

This was unanimously agreed to.

The PRESIDENT: I have now the pleasure to call upon Mr. William Preece to favour us with his lecture on "The Connection between Sound and Electricity."

Mr. W. H. PREECE: Mr. President, a late member of the present Ministry, at a dinner given by the Institution whose hospitality we experience in this hall, implied on the authority of one of the leading members of the engineering profession that invention, like cocktails and Colorado beetles, had taken root in America and had deserted old England. It is therefore to me, as I am sure it is to you, a great gratification to have brought before us an invention which is the offspring of British soil. During the last few months the science of acoustics has made marvellous and rapid strides. First of all we had the telephone, which enabled us to transmit human speech to distances far beyond the reach of the ear and the eye. Then we had the phonograph, which enabled us to reproduce sounds uttered at any place and at any time; and now we have that still more wonderful instrument which not only enables us to hear sounds that would otherwise be inaudible but also enables us to magnify sounds that are audible; in other words, the instrument which I shall have the pleasure of bringing before you to night is one that acts towards the ear in the same capacity that the microscope acts towards the eye.

I may point out in the first instance that the telephone and the phonograph depend essentially upon the fact—and a great fact it is—that the mere vibration of a diaphragm can reproduce all the tones of the human voice. In the telephone the voice is also made to vibrate a diaphgram, which by completing an electric circuit, or by varying a magnetic field, or by altering the resistance or electromotive force of the circuit, produces effects at a distance which result in the reproduction of the motion of the diaphragm; but in this new instrument diaphragms are cast aside, and we have the direct conversation of sonorous vibrations or sound waves into forms of electrical action.

Now if it had been the habit or custom of this Society to give to the papers and discourses delivered here sensational titles, I should have been inclined to call the few remarks I am going to make to-night "A philosopher unearthed." Professor Hughes is well known to us all. He has been more or less associated with this Society since its first inception. Whenever he is in London he is always amongst us. His instrument is well known to us as one of the most exquisite pieces of mechanism ever invented, and his works, though few, are known because they are sound. The chief characteristic of this philosopher whom I have succeded in unearthing is his extreme modesty. If he had been left to himself I do not think we should even have had the microphone here, but by a lucky chance he recently admitted me into his secret, and I am enabled to-night to bring before you the results of his labours, and they have been labours indeed. For months and months he has been working and striving at the ideas which at last he has elaborated into the microphone.

Now, the chief characteristic of the apparatus I am going to introduce to you to-night is its great homeliness, its uncouth roughness, and its absurd simplicity. With common nails, with small pieces of wood, with halfpenny money-boxes, with plain sealing-wax, with the ordinary apparatus which every child has at its command, he has been able to attack Nature in her stronghold, to ask her questions, and receive back answers, and lay bare to us, facts and thoughts which, though they have existed from time immemorial, are brought to light now for the first time.

Now let us, in the first place, ask ourselves this question.

What is sound? It is a very difficult question to answer in the short time at my disposal; but it is necessary I should first say something to you about the nature of sound, and then say something about the nature of electricity, and show you how the one can be converted into the other.

Now, what is sound? While I am speaking to you I am setting the air in this room into vibration. The air of this room is composed of an infinite number of infinitely small molecules: every molecule is set in motion and vibrates to and fro, backwards and forwards, like the bob of a pendulum, and between my mouth and every one of the ears in this hall there is a rapid but short excursion to and fro of every single molecule that composes the atmosphere of this room, and it is the impinging of these molecules against the drum of the ear that produces that sensation called *sound*. But more than that, not only is the air of this room in this marvellous state of motion, but every piece of wood, every wall, every picture—everything in this hall at this moment—is almost I may say alive, trembling away, moving backwards and forwards, forming what are called sonorous vibrations. If the sound be loud enough and the note deep enough we can distinctly feel these vibrations. Sound is therefore the vibration, in particular periods and particular phases, of matter.

Now, what is electricity? Faraday, the greatest electrician perhaps that ever lived, was asked that question, and he said the more he studied electricity the more he unravelled its mysteries, the more mystified he became as to its source and its origin; therefore it seems an act of impudence on the part of myself or anybody else to attempt to answer the question—what is electricity? But great strides have occurred since the days of Faraday; we know a great deal more now of the internal molecular action of bodies: we know that light and heat and sound are the mere action of those molecules of which matter is composed: and we feel sure from the facts brought to our notice by the delicate apparatus of the present day that electricity is simply a mode of motion, nothing more nor less than the simple play of the molecules of matter. The truth of this will be made evident to-night by the wonderful connection which exists between sound which we know to be a mere mode

of vibration, and electricity, which will reproduce to us the effects of sound.

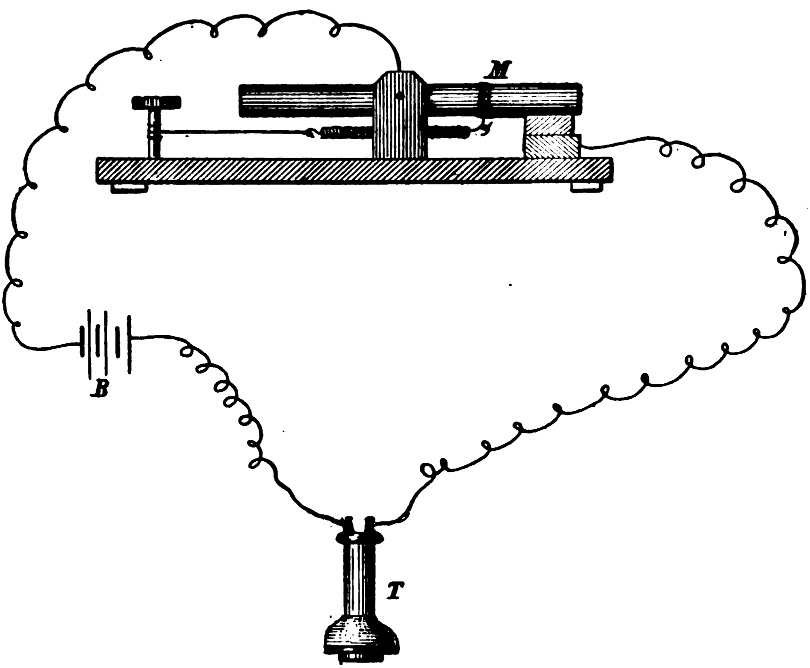
To make this evident to us, we must have two instruments—the one a detector, which will render apparent to us any electrical action that shall result in sound, and it fortunately happens that this marvellous telephone is an instrument of such extreme delicacy that it has made us acquainted with *currents* of electricity hitherto unknown, though their presence has been suspected. The telephone which Professor Hughes has used in his researches is as simple in its construction as are all his other apparatus. It consists of two rough pieces of board clamped together. There is half the end of an electro-magnet that probably has been in his possession since his early experiments, to judge from its appearance. The magnet is a piece of steel rod that has been magnetized. The wire used, and which he has found extremely useful to him, is wire that was originally made for very different purposes—for ladies' bonnets—and in front of this is placed a piece of ferrotype iron, well known by those who have experimented with the telephone.

But what is the source of sound? It was necessary in making these experiments that he should have a source of sound—something which would generate those sonorous vibrations of which I have spoken. This source of sound was a small mantelpiece clock of French manufacture, which cost originally three or four francs. It has been in use many years, and has been in many parts of the world. It is repaired with great lumps of sealing wax, but nevertheless it has, or ought to have, a pendulum, which gives a succession of beats, and those beats form a source of sound. Now, with this source of sound, and this beautiful scientific apparatus, the telephone, as a detector, he started upon one of Sir William Thomson's discoveries, viz.: that wires alter their electrical condition when they are placed under strain. He took a piece of wire, applied weights to it, connected the clock with it, and heard nothing. He was not disconcerted: he applied weight after weight till he reached the breaking strain of the wire, and at the moment when the wire broke he heard a rush or sound which he thought was an indication of what he was searching for: so he took the two ends of his wire and laid them together, placed his sources of sound above

them, and to his intense delight heard—what imagination perhaps assisted him in believing to be—a tick. He thought he was on the right track, and he then manufactured, with a flat piece of brass for a lever, a pin for an anxle, sealing wax for cement, black wax for solder, and the uncovered bonnet wire for binding, a little apparatus which enabled him to apply constant pressure to the thing he was experimenting upon: in fact, by this means he was enabled to produce what we electricians call a "bad joint."

To his intense delight he found with this bad joint and rough application of pressure, by putting various things, pieces of metal, chain, &c., underneath this little contrivance, he was able to obtain sonorous effects. But this contrivance, simple as it appears, was a great deal too elaborate and complicated for his purpose, so he took two French nails—little bright nails, so much used in France—laid them side by side, not touching each other, and bringing the ends of the wire in contact with them, and laying between or across them a third and similar nail, he was able to reproduce almost perfectly the sound of the clock, and, more than that, he began to get indications of the sound or tone of the voice. He then used chains; he took a gold chain and put it beneath his little compressor, and with that he was able to speak with great ease; from that he tried filings, and found with matter in a finely divided state he was able to reproduce all the effects of sound. At last he made little glass tubes, about two inches long, filling them with white bronze powder which artists use, which is a mixture of zinc and tin, and he was able to reproduce exactly the tones of the voice. But in his experiments with carbon he was able to make what may be called quite an independent and another discovery. The carbon he experimented with was the common carbon used by artists in sketching their drawings, and this carbon he found to be a non-conductor of electricity. The idea struck him that this non-conductor of electricity might be made a conductor, and by various processes he at last arrived at a plan of boiling or heating this carbon in quicksilver. Carbon so heated in an atmosphere of quicksilver itself becomes permeated through and through with quicksilver, and by that means we get the mercury subdivided into an almost infinitely pure state. Probably mercury

in this state as closely approaches the molecular as anything can do; there is no apparent indication of mercury under the microscope, and yet we know that the carbon has been mercurised, because it is converted from a complete insulator into a conductor, and it has a metallic ring when it falls. Now, then, having by these processes arrived at a substance which is remarkably sensitive to all the variations of the sound of the human voice, his next task was to construct these things into such a form as to make them telephonic transmitters. For that purpose he brought to his aid a very cheap kind of apparatus, a halfpenny money-box; inside this he placed his carbon transmitter, and as this discovery is not fenced in by fear of the patent or any other law, I am quite sure you will be glad to know how to make a Hughes' transmitter. First, he takes a piece of ¼-inch board about 2 inches long and 1 inch broad, and he raises upon that two thin brass bearings with a hole worked through by means of a pin for the support of his axis; he then takes a piece of carbon about 2 inches long, which has been mercurised, which has a pin cemented to it, near its centre, which



acts as an axis, and makes it into a lever. On the board he places another smaller piece of carbon similarly treated, and upon this rests another similar sized piece of carbon, the two being connected by a piece of paper. (See Fig., in which M represents the transmitter, B the battery, and T the telephone.)

Now the battery is another remarkable specimen of scientific manufacture. Three little glass tumblers are taken; at the bottom of each a piece of copper wire is coiled spirally. The copper wire is covered with a little sulphate of copper. The tumbler is then filled with moistened clay, and upon the top of this clay is placed a piece of scrap zinc. The three cells are placed in a cigar box. The tube-transmitter consists of a glass tube about 2 inches long and ¼-in. diameter, inside which several pieces of mercurized carbon are inserted, touching each other with a pressure regulated by a screw fixed to each end. The size of the carbon appears to be of little consequence. He has produced effects with carbon not larger than the head of a pin. We shall show this by-and-bye; but rather than disturb the order of these researches I think it advisable in the next place to show you how this principle has been carried a little farther, to produce what he calls the microphone. This apparatus is extremely sensitive. It will give evidence of nearly any sound; but in the microphone itself, which I have here, this extreme sensitiveness is carried to a still further extent. In point of fact this is a microphone; but in this particular instance the pressure bearing between the two carbons is regulated by a spring, and it is so regulated that the transmitter is independent of any position in which it may be held. It is free to be moved in any direction, in consequence of the pressure of the spring; but in one form of instrument this spring is dispensed with, and the pressure between the carbons is reduced to its greatest sensitiveness by making the two arms of this lever as short as possible; but in the first machine he used, a piece of carbon was fixed on the top of an upright board of that kind, and a smaller piece was fixed down below. A cup-shaped ball was made at one end, and a similar one at the bottom, resting upon a piece of lozenge-shaped carbon; and this lozenge-shaped piece of carbon rests with the greatest nicety upon its lower disc, and is just

in that position of equilibrium that the slightest atmospheric disturbance produces the effects which we are now about to show you.

I think it desirable to tell you that you must not expect to-night distinct articulations. We have made a violent effort to make these experiments evident to you all. [*Illustrations were here given of speaking, singing, &c., and were heard all over the hall.*]

Now, the effects you have just heard have been produced by a microphone similar to that drawn on the board. We will now repeat the effects with the machine on the table, and in order that you may judge of the effect—for Professor Hughes desires you should see there is no deception—we will connect this up, and use his old friend the clock to make its ticks, if it will, evident over the whole room. One of the greatest effects which this instrument produces is to render evident the tramp of a fly, and we have some nice little captives with which we will demonstrate that effect at the close of the meeting. [*Illustration with clock.*]

To show that the sounds come from the clock itself Professor Hughes will lift up the clock, when all traces of sound will have disappeared, and on putting it down again the sounds will be produced; so that the sound you hear is the sound of that clock which has been magnified.

Now, we have here a common quill pen, and Professor Hughes will do as they do on the stage, pretend to write a letter, and I have no doubt if you listen attentively you will hear the scratching of his pen. [*Illustrated.*]

Now, there are some peculiarities in this apparatus that are very striking. In the first place, though the sounds produced are very great, they do not interfere with each other. If you have a friend at the other end speaking to you, you can hear his voice distinctly working through your voice, and the result is you get a duplex action. Two or three persons can talk to each other without impediment or confusion.

Yet another point is, that the articulation is absolutely perfect. One of the great difficulties both in the telephone and the phonograph is getting the noises and sibilant sounds reproduced, such as "s" and "c" and "sh," &c., which are produced by such extremely

minute variations in the sonorous vibrations that they are lost in those instruments. Thus, if through the telephone you ask a person to waltz, it will come out "walk," and names like my own with the sound of "s" in it, though it is spelt with a "c," would come out "Pree"—not Preece. In this transmitter one of its chief peculiarities is the fact that all sounds are faithfully reproduced, and it tends very much to upset the notion—Helmholtz' theory—that vowel sounds and other sounds are due to the superposition of waves upon waves of tones and overtones. This apparatus shows almost unquestionably that all these different properties, all these effects of intonation, are due to differences in the form of the wave sent. Another peculiarity is this: I have told you that all in this room, every one's body while I am speaking, is alive with sound. If you take this transmitter and place it in front of your mouth, or put it on your forehead, or on the top of your head, or put it in your pocket, or upon your breast, it will still transmit sounds to distant places. Put it in a room, it does not matter where, it will reproduce the sounds. Put it anywhere in a drawing-room where there is a piano, you will hear the sounds of the piano faithfully reproduced. It is, as you see, a marvellously rough affair; you may throw it up, kick it about, and do what you like with it; it will always act. Here is the identical box that Professor Hughes made two or three months ago; it has never been touched; it has been always at work, and never needs repair.

These are some of the peculiarities of this instrument, and I dare say some of you would like to know a little about its theory. We have here two points in contact, and those two points in contact complete an electrical circuit. The electric current that flows through that circuit depends for its strength entirely upon the obstacles or resistances in that circuit to the flow of the current. Any alteration in any shape or form in the resistance of that circuit will result in the increase or decrease of the strength of the current flowing, and upon this board I will make a rough attempt to endeavour to give you an idea of what occurs. You must not conceive that these round balls are molecules themselves; they are merely meant to represent the sphere of action of each molecule at each point of contact. In a normal state the molecules rest against

each other as shown by the upper line. When from any cause pressure is increased, they are contracted, as shown in the second line; when from any cause the pressure is decreased they expand in the form shown on the other line. While I speak at you, the air of this room is thrown into vibration, the mass of air being subdivided into molecules in compression and molecules in extension. In a long wire these successions of compressions and extensions compensate each other; but when we break up a body into infinitely small parts, when we make contact between two bodies as shown there, we isolate the portion of the sonorous wave in compression from that in extension, the result is that we have a variation in the resistance of the line. Now this variation in resistance depends upon the compression and dilatation of the molecules. It depends upon the tone of the voice, and the result is the resistance of the current varies with its variation of pressure, and at the distant end we have currents varying exactly as the voice varies, and reproducing on the telephone all the effects which we have seen. Hence follows the action of the microphone, which depends upon the variation produced in the contact of bodies by the sonorous vibrations of the voice. As I am now speaking at that microphone all the molecules of that transmitter are thrown into this elaborate series of compressions and dilatations. The current is varied; the current goes to the room below and is reproduced upon the telephone as we have heard. Hence the effect is due to the difference of pressure at the points of contact, as is proved by using atmospheric pressure, applying heat, making noises, and any large variation of pressure results in sound being reproduced.

No one has ever been nearer a great discovery than Mr. Edison. His telephone is based on the variation of resistance in carbon due to pressure. He used carbon and finely divided matter, but he worked on the idea that the difference in pressure was produced by the vibration of a diaphragm. Had he thrown away his diaphragm he would have forestalled Professor Hughes in this respect, and found that the sonorous vibrations themselves produced this difference of pressure.* The great secret of Professor Hughes's discovery

* It appears that Mr. Edison actually did this, but he was anticipated by Professor Hughes.

is,—that sonorous vibrations and electrical waves are to a certain extent synonymous.

Now, as to the uses to which this instrument is capable of being applied. It has been applied to surgical purposes in the form of the stethescope. Though it does not show very markedly the beats of the heart, because they are more mechanical thumps than sonorous vibrations, yet it will show the injection and ejection of air in the lungs, and for many other surgical purposes it must become a valuable instrument. It admits us to some of the mysteries of insect life, and by its means we can hear sounds emitted by insects which have never been heard before. Going further, it has enabled the deaf to hear; deaf persons who never heard a telephone before have been able to hear distinctly. It has enabled us to hear the physical operation which goes on in the process of crystallization of bodies and other things which before were wholly inaüdible, and in fact it is impossible to say to what uses it may not be put.

It is rather remarkable that in an excellent paper read before the American Electrical Society the author, Mr. Pope, makes these curious remarks:—

"The most striking results are to be looked for in the direction first pointed out by Mr. Gray, for the reason that if an effectual method of controlling the resistance of the circuit by means of atmospheric vibrations can be discovered, the source of power, which in this case is the battery, may be augmented to any required extent. It is not to be denied that the problem thus presented is one of exceeding mechanical difficulty, but there is no reason to suppose that it may not be successfully solved. It is to the development of this variety of speaking telephone rather than to that of the magneto instrument that inventors will find it most advantageous to turn their attention, for I hazard little in saying that the latter has already reached such a surprising degree of efficiency as to leave comparatively little more to be done within the necessary limitations which have been pointed out."

Mr. Pope throws out as a suggestion what has now been done with the exception of the supposed mechanical difficulty, and that has been got over by a halfpenny money-box.

Now, one very pleasing and gratifying circumstance attaches to this discovery of Professor Hughes: he has thrown it open to the world, and by that means he has no doubt checked that species of immorality—I don't know what else to call it—connected with the infringement of the patent law as regards the telephone. He allows us all to manufacture microphones for ourselves; but even he has been subject to a rather peculiar incident. One impulsive and active gentleman, who was present at the Royal Society the other night when Professor Hughes first described his invention, went home and made himself a microphone, wrote a description of it, and sent it off post haste to Paris. A short time afterwards, Professor Hughes himself, with great care, prepared a paper to be read before the French Academy, but to his great surprise he found that he had been forestalled—a description of his instrument had already appeared in the Paris prints from the gentleman in question.

There are lessons to be learnt from this discovery; and the principal lesson is—we can all of us, with the means at our disposal, cross-question Nature and find out her secrets; and there are many secrets which yet remain to be divulged. We learn the wonderful connection which exists between all the physical forces: heat and light, and electricity and magnetism, are all co-related; and it has come to this, that what boys have said in joke has come to pass in earnest. We have been able to convert electricity into light and light into electricity. We are now able to convert electricity into sound and sound into electricity, and thus we are enabled to see the thunder and to hear the lightning.

The President: We have listened to a very interesting and instructive address, and I hope it will be followed by an equally interesting discussion. I see many here who are fully able to take part in it, and, with your permission, I will call upon His Grace the Duke of Argyll to open the discussion.

The Duke of Argyll, K.G., rose and said: Mr. President and Gentlemen—Though I am a stranger here, I am very glad of the opportunity which the kind invitation of your President gives me to say a few words as to the extreme interest with which I am sure

we have all heard the admirable lecture just delivered by Mr. Preece —one which, for interest of matter and for clearness of exposition, I have never heard excelled in the course of my experience of scientific lectures. It is quite obvious that we are here in the presence of one of the most remarkable discoveries of an age which is full of discoveries—discoveries which are sure to be utilised in a thousand ways which at present we cannot foresee. Mr. Preece has said at the beginning of his lecture that he rejoices on this occasion at least that the progress of this discovery has taken place on English soil. On further inquiry, however, I am not sorry to find that the distinguished man whose discovery has just been explained is, although a resident in this country, a citizen of America, because, though I have as strong a patriotic feeling as any gentleman present in this assembly, I am one of those who, in matters of science, are thoroughly cosmopolitan in the triumphs of human intellect by whatever nation they may be made—and further, I am one of those who feel that the citizens of America are, after all, our fellow-countrymen. Now, with regard to the uses to which this discovery may be put, there are many which immediately suggest themselves to one's imagination.

I am sorry to hear, from Mr. Preece, that the immediate application of it, in so far as it has yet been tried, to the sounds of the human heart, does not throw much additional light on that most curious matter, the circulation of the blood, because probably the sounds are of a very muffled nature, and do not give rise to sonorous waves such as those which have been explained to us; but I cannot doubt for a moment that one of the most immediate applications of this very valuable discovery will be a surgical application, and that through it our medical men will be able to pronounce upon a series of diseases with which we are at present wholly unacquainted. Now, being myself accustomed to think a good deal about political affairs, I confess I was struck with some of the inconveniences which might occur through the agency of this discovery. We are now very close, you know, to Downing Street, and it occurred to me if you placed one of these little halfpenny boxes in the room where the Cabinet sits we should have the whole of the secrets of the Cabinet revealed in this hall of the Civil Engineers (laughter); or

if by any extraordinary ingenuity, such as many conjurors can command, one of these little boxes could be inserted in the pocket of my distinguished friend Count Schouvaloff or Lord Salisbury, I have no doubt we should be at this moment in possession of all those secrets which the whole of this country and of Europe are desirous to know. I should like to know from Professor Hughes that there is some means of preventing this. I remember a lady friend who was very fond of attending sermons, and she was laid up by a complaint which prevented her going to church; but she contrived to take a lodging almost under the roof of the church where a celebrated minister preached, and she had a speaking trumpet fixed near the pulpit, and by that means she was able to enjoy the sermons she so much desired to hear. But with this instrument of Professor Hughes we shall be able to poach on every one's manor and know every one's secrets; and I should much like to know from Professor Hughes whether—since one of these little boxes set anywhere in a room is capable of transmitting to a distance all the conversation which takes place in that room—he is able to provide an antidote for it.

Dr. Lyon Playfair, M.P.: I can only sympathise with the remarks which my noble friend the Duke of Argyll has just made to us. In fact one sees no end to the application of this discovery. I have no doubt before long Professor Hughes will connect his instrument with the aerophone which magnifies sounds so that one can hear it over four or five square miles. Only conceive what might happen in a debate in which such orators as the Noble Duke who has just sat down or Mr. Gladstone made one of their magnificent speeches: supposing one of these instruments were connected with the aerophone, and instead of those speeches being confined to the area of St. Stephen's they were sent over four square miles of London, and allowed millions of the public to enjoy the beneficial effects of that eloquence! It is not only possible but probable. Only conceive to what large influences such a discovery as this may lead. Or supposing you connect this instrument with the Opera house, where one of our great contraltos or sopranos are singing, it would be possible by means of the aerophone to communicate it to a whole town. The development of this instrument in

connection with others already in existence may be productive of prodigious effects. I think in the hands of the physician it will prove to be a discovery of the greatest importance and value. It occurred to me that though the muffled sounds of the heart might not be adapted for repetition it might be possible in acute diseases, such as inflammation, or other acute diseases which proceed from too energetic action in particular parts of the body, it occurred to me that these might be detected by this agency. In the beginning of a discovery of this kind it is impossible for the most lively imagination to see the results that may flow from it; and in the hands of Professor Hughes, or in the hands of others, it is impossible to place a limit to the development which may result from the description we have heard to-night. I consider it one of the most remarkable discoveries of modern times, and I congratulate Professor Hughes upon it. He is a citizen of a country to which I am dearly attached by many acts of kindness shown to me; at the same time he has been so long connected with this country that we may fairly claim this as the joint discovery of two great nations.

Mr. Willoughby Smith: Marvellous discoveries have been brought before us lately in such quick succession that I am pleased we have at length an opportunity to pause and reflect on what we have already seen and heard. I cannot agree with Mr. Preece in his molecular and sound-wave theory of the "Microphone." I believe the phenomena to be solely due to what is known as a varying contact, and that the "microphone" is nothing more than a sensitive commutator. Mr. Preece has told us that by the aid of the microphone the tramp of a fly can be heard, resembling that of a horse walking over a wooden bridge; but I can tell you something which, to my mind, is still more wonderful, that by the aid of the telephone, I have heard a ray of light fall on a bar of metal; this may appear a startling statement, but with the whole facts before you it will become extremely simple. I have on a previous occasion shown that the electrical resistance of selenium is very much reduced immediately it is exposed, either to solar or artificial light, consequently if a bar of selenium, excluded from the light, form part of a circuit in which are included a battery, a telephone, an electrical bell, and a gal-

vanometer, nothing will be heard until the light be allowed to fall on the selenium, which is done simply by raising the lid of the box in which the selenium is placed, when immediately sound is heard in the telephone, the bell rings, and the needle of the galvanometer moves, simply because the resistance of the circuit is reduced, and consequently the electromotive force increased. Now, when I read that Professor Hughes had discovered that bodies were susceptible to sound as selenium to light, I removed the selenium from the circuit, and substituted every substance I could think of in its place, and used every conceivable means to produce sound, but without effect. But if, in a metallic circuit, be placed either a loose contact or a sensitive commutator, then one can produce all the phenomena claimed for the microphone. I have obtained very satisfactory results from three very fine "rat-tail" files placed in the circuit, similar to the letter H, one end of each of the side files being fixed, and the other file simply lying across them; the longer the side files, and the nearer the cross one is placed to the unsupported ends of the other two, the better; if the cross file be replaced by loose threads of metalized silk, and a spirit lamp be placed beneath the same, so as to create a draught, then is heard in the telephone a noise similar to the sound of the burning wick of the lamp, especially when first lighted. What Professor Hughes has discovered is that, by allowing the breath or other motive power to effect a sensitive loose contact or commutator, contacts of varying duration are made, which are reproduced in corresponding sounds on that marvellous instrument the telephone; this is an important discovery, and Professor Hughes has my hearty congratulations.

Mr. LATIMER CLARK: I did not intend to make any observations, and I feel almost out of place, after the eloquent addresses we have heard, in going back to the ordinary technical view of the subject; but I would just remark that Mr. Preece, has, I think, omitted to mention one of the experiments of Professor Hughes. The little glass cylinder you see here [*exhibiting*], with small segments of willow wood carbon dipped in mercury, is extremely sensitive to heat, so much so that the approach of the hand or other warm body causes very great variation in the resistance of the circuit,

and also causes a noise in the telephone. So does a slight alteration of the pressure on the end: if you press on the end of the tube you cause a noise in the telephone, and produce great deflection of the needle of the galvanometer. You might make a sensitive relay by causing a needle or an electro-magnet to slightly compress the segments in this tube which would doubtless be more sensitive to delicate currents than any relay we have at present. I therefore throw out the suggestion in order to make it public. I also think a very excellent Morse sounder may be made from the microphone by causing a continual sound to be produced in the telephone, and it is obvious that one source of movement would cause any number of instruments in an office to produce continuous sounds, the signals would be received on a relay and would come out, not as dots, but as dots and prolonged dashes which could be more accurately and easily read than the ordinary sound signals.

Lord Lindsay, M.P., F.R.S.: I will only mention one experiment which struck me as being interesting. I constructed one of Professor Hughes's little instruments from a description which I read of it, and having stretched a piece of parchment over a circular frame so as to form a vibrating membrane, I attached to its centre a piece of carbon which was connected in circuit with a battery by means of a fine copper wire. Against this piece of carbon I placed a second similar piece, so arranged that I could increase or diminish its pressure upon the first piece by means of an adjusting-screw, and to this piece was also attached a copper wire by which it could be placed in circuit with a battery and telephone and with the other piece of carbon. Under the influence of sonorous vibrations the membrane vibrated, and in doing so produced a variation in the pressure, and consequently in the electrical contact between the pieces of carbon, reproducing the original sounds in the distant telephone. On one occasion I placed the apparatus on the sounding-board of a pianoforte, upon which my little girl was practising, she was playing scales, and I noticed a very remarkable effect. Whenever one particular note was struck, a harsh loud sound was heard in the distant telephone, although all other notes were clearly transmitted and reproduced. On examination I found that the note which produced this curious effect

was that whose period of vibration corresponded with that of the membrane, and in the sound produced could be heard the fundamental note and overtones, as well as the octaves and other sympathetic notes sounded by the strings of the pianoforte.

Professor GRAHAM BELL: No one can more heartily congratulate Professor Hughes upon his great discovery than I do: I think all those who investigate telephonic effects will realise that we have touched upon the threshold of a new science, and that we have discoveries yet in store for us. I must confess that the explanations of the action of the microphone afforded by Mr. Preece and Mr. Willoughby Smith are to my mind not perfectly satisfactory. There are certain points in the telephone itself that may lead us to the true solution of this problem. If you fix your attention upon the first diagram shown by Mr. Preece you have a constant source of electricity (the battery), a telephone, and this curious instrument. Now, the laws of audibility of the electric current by the telephone are analogous to the laws of induction. No sound is audible so long as your current is of uniform intensity. But the moment you change its intensity you have a sound from the telephone. Conversely when you have a sound from a telephone the intensity of the current passing through it has been changed; and the loudness of the sound is an index of the amount of change. Now, unless the microphone were itself a source of electricity, it is evident that the only way in which it can change the current is by varying the resistance of the circuit. So far, we may be perfectly certain that the action of this instrument depends upon the variation of the resistance; and the only question is as to how this variation of resistance can be accomplished by the influence of such feeble sounds as those produced by the escapement of a watch or the footsteps of a fly. As the sounds produced from the telephone are quite loud, the current must change very greatly in intensity, and the resistance of the microphone must vary in a most extraordinary manner, probably being at its maximum many times the resistance of the rest of the circuit, and at its minimum, but a fraction of it. Mr. Preece seems to consider that the molecules of the substance composing the microphone change their shapes when subjected to compression by a variable force; and he

has figured to us his conception of the alternate contraction and expansion of a chain of molecules under the influence of sound. Mr. Willoughby Smith, on the other hand, attributes the effects produced by the microphone to imperfect contact. He supposes the carbon points to rattle against one another under the influence of a sound, and he seems thus to regard the microphone as a delicate form of interrupter. I am more inclined to accept Mr. Willoughby Smith's explanation than that given us by Mr. Preece, but the reproduction of articulate speech negatives the assumption that the microphone acts as an interrupter. The possibility of producing electrically any given quality of sound depends upon the possibility of producing a current of electricity the intensity of which varies in a manner proportional to the varying velocity of a particle of air during the production of that sound. Hence, however much the points in contact may rattle together, the circuit must not be broken or the effect of quality will be lost and a mere noise be produced instead. If I might offer a suggestion I think it probable that there is a variation in the *amount* of contact. Let us suppose an enormous number of particles to be in contact (each singly offering a high resistance to the passage of a current, but conjointly offering very slight resistance), then when the microphone rattles under the influence of a sound, a larger or smaller number of particles come into contact and the resistance of the microphone changes without absolute break of circuit. Still, we must not forget that at each increase of resistance heat must be produced at the points of contact, and it is probable, nay, extremely likely, that heat has a great deal to do with the results obtained. I was experimenting with a microphone the other day, and it kept on vibrating on its own account for nearly two minutes. A clear musical tone was emitted by the telephone in circuit with it. The microphone always exhibits a tendency towards vibration on its own account when it is adjusted most delicately. The instrument, as I constructed it, consisted of a rod of charcoal placed horizontally with its ends resting upon two other rods of charcoal. This arrangement, when shown to Professor Dewar, at once suggested to his mind Trevelyan's bars. I think it not at all unlikely that the microphone is in reality a species of Trevelyan's rocker, and

that it may thus be in a condition to be affected by feeble sounds. It is quite conceivable that sounds too weak by themselves to throw the mass of the microphone into sensible vibration may yet be sufficiently strong to start and control the vibrations due to heat, or at least it is conceivable that heat may assist the action of the sound. For instance, suppose the charcoals rattling together under the influence of a sound. Now, as the charcoals separate, the resistance of the microphone increases, the points of contact become heated, and the expansion due to this cause occasions a greater separation than would otherwise take place. Thus, heat may co-operate with a feeble sound to produce a greater amplitude of vibration at the points of contact than would be produced by the sound alone. Those who are interested in this subject would do well to read Professor Page's paper upon "The Vibration of Trevelyan's Bars by the Galvanic Current."* The microphone recalls vividly to my mind some of the illustrations in this paper. Whatever may be the true explanation of the effects produced by the microphone, there is evidently a vast field opened before us for inquiry. I will not take up your time with any further remarks, but will merely again offer my congratulations to Professor Hughes upon his discovery.

Professor Hughes: It seems that the description of the microphone is not well understood. I can excuse that because it is as much as I can do to understand it myself; but, as far as my experiments have gone, they show that the effects produced are the result of the increase and diminution of the resistance. Mr. Willoughby Smith designated it a commutator, and attributes the effects to contact. That is quite right, only it is not from the point from which I view it, and that is the way in which the effects differ.

If we take a board like this [*illustrating*], and give it a blow here, that nail will jump up from the transmission of force. The same thing takes place in acoustic properties. A blow is simply rapid pressure, as distinguished from hydraulic pressure: the difference between a blow and hydraulic pressure is one of rapidity. If you take a piece of glass and put sand on it, and strike the glass, the sand will jump away and form figures. So it is here. [Professor

* Silliman's Journal, 1850, ix., pp. 105-108.

Hughes illustrated his views by means of diagrams on the board, showing the curves of articulated sounds, both in speaking and singing, &c.]

The PRESIDENT: As time is advancing, and as Mr. Preece, I believe, has some further experiments to exhibit at the close of the meeting, I will make a few observations only on the very interesting matter which has been brought before us. The discussion that has taken place is remarkable for the excellent temper which has been shown between two great rival discoverers. I think all of us must have been pleased to have seen how these two gentlemen, Professor Bell and Professor Hughes, have described and brought before us their particular views regarding certain actions in the two instruments, the telephone and microphone, which, when we come to compare them, will be found to have many points of analogy, and though essentially different in detail tend towards the accomplishment of the same important end. Mr. Preece and Professor Bell differ with regard to the action which takes place in the microphone, and Professor Hughes favours naturally the views which Mr. Preece has expressed; but I think there is probably not so much difference between those two views. It is quite evident that the action of the microphone is due to variation in electrical resistance produced by vibrations in an imperfect conductor, such as carbon, or an aggregate of divided pieces of metal, and the question for consideration is how this variation in resistance is effected. When two pieces of carbon are pressing one upon the other, and vibration is imparted to one of them, it is easily conceived that in consequence of this vibration the pressure between the adjoining points of the carbon will be modified, and in consequence of such variation in pressure, the electric conductivity of the carbon is also influenced, whilst according to Professor Hughes' explanation, the cause of variations in the electrical resistance must be looked for in the lateral increase of points of contact.

We have another discoverer who has already thrown light upon this subject, viz., Mr. Edison, of New York, the well-known discoverer of the phonograph, who, in constructing a form of telephone of his own, introduced carbon contact, which gave him resistances variable with the amount of physical pressure he brought to bear

upon the carbon; and I must say that this question of varied resistance due to vibration will probably resolve itself simply into a question of pressure between particles of matter which are conducting in themselves, but which are held so lightly in contact that pressure is needed in order to establish conductive continuity.

I should have liked that something more had been said of this discovery of the microphone, with reference to its two elder sisters, the telephone and the phonograph, being of opinion that the three are only separate steps in the achievement of an advance in physical science which bids fair to be considered hereafter as one of great moment, not only as regards telegraphy, but as a means also of affording a more perfect insight into the nature of molecular action. We have heard from Mr. Willoughby Smith that in substituting crystalline selenium for carbon in the microphone, a ray of light produces an effect analogous to mechanical vibration, and announces itself in a report comparable to a clap of thunder, and I can quite follow him in his arguments with respect to the matter. His Grace the Duke of Argyll alluded to the application of this discovery to physiological research, and I could have wished that some of our learned physicians had taken up this point in the discussion, because I believe myself that the influence of these discoveries upon physiological research will be very great indeed. One thing has occurred to me in considering these matters, which I will take the liberty of mentioning. We have the remarkable effect of the phonograph producing a record of sounds simply from the indents given to a slip of tin foil, which record can be reproduced at any time. This strikes me as being an exceedingly analogous case to the impress produced upon the brain by what we hear and see. An impress is produced, which, for the moment seems lost, but which, in a vigorous mind can be reproduced at any time. Now, the faculty of memory is not conceivable on any other hypothesis but that of a mechanical record being left on the brain and stored up for perhaps half a century to be restored in the succession in which it has been laid down; otherwise how could the human mind reproduce impressions imparted years ago at will, or how could they be involuntarily revealed in our dreams? The discoveries which are now brought before us will undoubtedly

serve to increase our stock of knowledge on physiological and metaphysical, as well as physical subjects, regarding molecular action, of which we have hitherto had but very imperfect indications, and I think we cannot be too thankful to those gentlemen who have enabled us to discuss them as we have done, and I will therefore move that a hearty vote of thanks be given to Mr. William Preece for his communication. I think our thanks are also due to the two discoverers who are here to-night, and have given us the benefit of their views. I therefore propose that we also return our thanks to Professor Hughes and Professor Graham Bell.

The votes of thanks were unanimously accorded.

Mr. W. H. Preece: My remarks will be very few. As I said, when I spoke on the theory of the thing, I had not time to enter into it. In point of fact there is no real difference between the views of Mr. Willoughby Smith, Professor Graham Bell, and my own. The only difference is difference of language. Most differences in scientific controversies are attributable to difference of language. Whether variation of current is due to difference of contact or difference of pressure, matters very little, provided we use contact and pressure to imply the same thing. That which I call pressure others call contact, and that which Mr. Willoughby Smith calls contact I call pressure. It has afforded great pleasure to me to bring this matter before you. I have had, during the last twelve months, the pleasure of introducing before you the Telephone, the Phonograph, and now the Microphone, and I hope next year when we meet again it will still be my pleasure to bring forward other new inventions and discoveries. I was anxious that Professor Hughes should have brought this subject before you himself, but if I had not undertaken to do so you would not have had it. Beyond assisting Professor Bell, Mr. Edison, and Professor Hughes in any way I could, I can claim no merit of my own either in respect of the Telephone, the Phonograph, or the Microphone.

The Meeting then adjourned to the 13th November.

ORIGINAL COMMUNICATIONS.

NOTE ON ELECTROLYTIC POLARISATION.

By Professors John Perry and W. E. Ayrton,

of the Imperial College of Engineering, Tokio, Japan.

The following experiments made in connection with the general subject of electrolytic polarisation (which we are now and have been for some time investigating) have so obvious a bearing on the tests suitable for determining whether the leakage from a faulty telegraph cable is at only *one* or at *many* points, that we have thought it desirable to communicate them to the Society before the completion of our second paper on electrolytic polarisation, designed to follow the one that has already appeared in their Journal.

The plan of the following experiments consists in sending a current with one or more Daniell's cells through a voltameter and measuring the current after a certain number of minutes, then suddenly reversing and observing the current a fixed time after reversal. A comparison is then made between the two currents so observed as one or both of the voltameter plates is altered in size, material, &c.

The results given in the following table are to be explained if we remember first, that, since the surface of the wire in the voltameter is extremely small compared with that of the plate, the gases deposited on the wire will have far more effect than those deposited on the plate in the polarisation of the voltameter; secondly, that oxygen deposited on copper will, on account of its forming an oxide, be less operative in producing a reverse current than hydrogen; while on the other hand, on account of the great absorption of hydrogen by platinum and of the difficulty of forming an oxide of platinum, it will be the deposited oxygen that will produce the chief polarisation in the platinum voltameter. When, therefore, the anode in the second set of experiments was a

Number of experiment.	Plates of Voltameter.	Number of cells in series used to send the current.	How joined to Voltameter.	Duration, in minutes, of current before taking reading and reversing.
1	2 copper plates of equal size	4	...	5
2	...	3	...	...
8	...	2	...	...
4	...	1	...	...
5	...	1	...	...
6	A copper plate and a copper wire.	1	Copper pole of battery to wire.	4
7	...	2	...	4
8	...	8	...	8
9	...	8	Zinc pole of battery to wire.	...
10	...	2	Copper pole of battery to wire.	...
11	...	2	Zinc pole of battery to wire.	...
12	A copper plate and a copper wire.	1	Copper pole of battery to wire.	3
18	...	1	Zinc pole of battery to wire.	...
14	A platinum plate and a platinum wire.	8	Copper pole of battery to wire.	3
15	...	8	Zinc pole of battery to wire.	...
16	...	1	Copper pole of battery to wire.	...
17	...	1	Zinc pole of battery to wire.	...
18	...	3	Copper pole of battery to wire.	...
19	...	3	Zinc pole of battery to wire.	...

Deflection.	Deflection 30 seconds after reversal.	Remarks.
42	50	About 15 per cent. *increase* in the deflection.
36·5	42	
27	31	
7·5	8	
7·5	8·5	
55	27	51 per cent. *diminution*.
72	54	25 per cent. *diminution*.
43	31·5	26 per cent. *diminution*. Both the readings in 8 being smaller than those in 7 is due to the adjustment of the galvanometer having been altered.
30	47·5	25 per cent. *increase*.
28·5	18·5	*Diminished* to ⅔.
10·5	29·5	*Increased* to 3 times.
12·5	7	Deflection roughly *halved*.
5	12·5	Deflection roughly *doubled*.
		Platinum wire and platinum plate made red hot between any two of the following experiments.
33	37·5	Small increase, *practically constant*.
21	47	Great *increase*.
1	15·5	Great *increase*.
9	1·5	Great *diminution*.
47	49·5	Small increase, *practically constant*.
40	57·5	Decided *increase*.

copper wire there ought to be a slow falling off of the first current and a quick falling off of the reverse current, consequently the second deflection ought (as it is found by experiment) to be smaller than the first. When the copper wire is the cathode there will be a quick falling off in the first current and a slow falling off in the reverse current, the second deflection in this case will therefore be larger than the first. With a platinum point and plate on the other hand the whole will be changed, the deflection being increased by reversal when the platinum wire is the anode and diminished by reversal when it is the cathode.

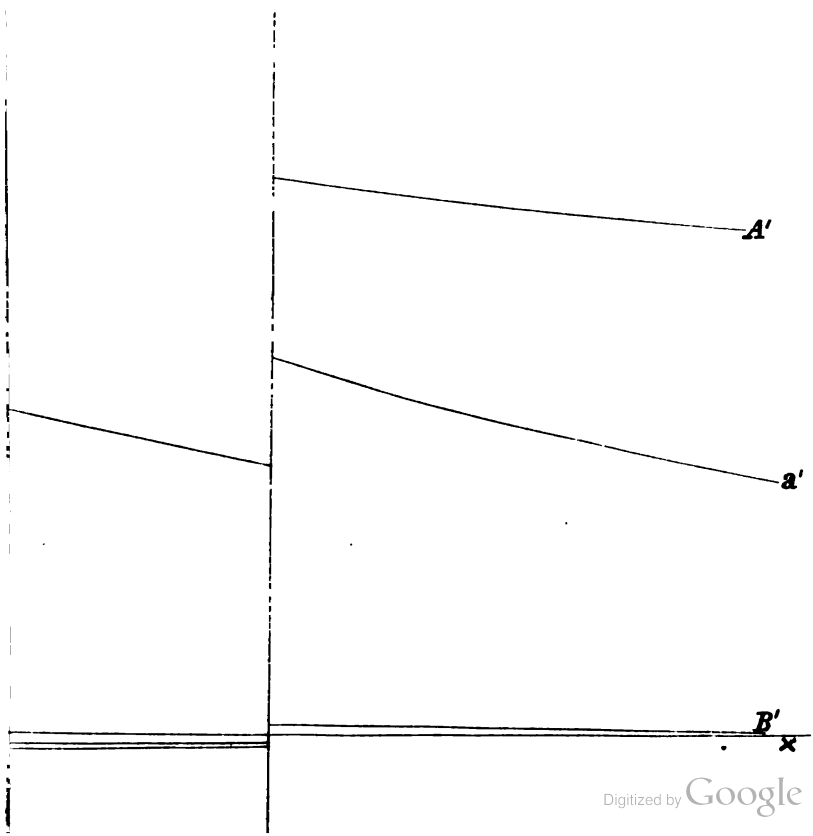
The greater effects produced by one cell than with several in series is due of course to the total electromotive force of polarisation being limited to about 1·7 volts, viz. that which will decompose water.

To ascertain whether the above explanation of the observed phenomena was correct, time readings, with a break-circuit-chronograph,* were taken of the currents in various cases, instead of merely a single reading before and a single reading after reversal. All the results were found to be consistent, and some of the curves drawn from time readings are shown in the accompanying figure.

The curve *A A A* shows the time fall of current when the zinc pole of one Daniell's cell was connected with a platinum plate and the copper pole with a platinum point immersed in water, current being measured parallel to *O Y*, and time parallel to O X. At 181 seconds, represented by *O P*, after the completion of the circuit the battery was suddenly reversed and the curve *A′ A′ A′* was obtained, the scale for current and time remaining as before. A repetition of this experiment gave the curves *a a a*, *a′ a′ a′*. The curves *B B B*, *B′ B′ B′* show the result of a similar experiment when the zinc pole of the Daniell's cell was connected with the platinum point. Between every two experiments the platinum plate and point were both made red hot to free them from any trace of previous polarisation.

* A break-circuit chronograph is a rapidly running Morse instrument in which the armature is kept held down by a current, except at the commencement of every second, when the current is broken by a clock. The moment of any event is noted by an additional break of the current produced by hand or otherwise.

NOT



We have here given the explanation which at once suggests itself of this very curious phenomenon, but it is given rather as a help to the memory than that we think the phenomena really explained. It is certain that with small electromotive forces no gas makes itself visible at the plates of a voltameter, and we think it probable that phenomena of the same kind as we have been describing, although perhaps much more difficult to explain, would occur if glass or any other dielectric separated the plate and point. It is also probable that there is a connection between this phenomena and the greater ease with which *negative* electricity comes in sparks from a pointed conductor.

JOHN PERRY.
W. E. AYRTON.

IMPERIAL COLLEGE OF ENGINEERING,
TOKIO, JAPAN, *March*, 1878.

THE RESISTANCE OF GALVANOMETER COILS.

BY PROFESSORS W. E. AYRTON AND JOHN PERRY,

of the Imperial College of Engineering, Tokio, Japan.

In connection with the winding of galvanometer coils, the three following questions have been solved:

2. What is the best resistance to give to a galvanometer coil of fixed shape and size when *one* gauge of wire *only* is employed in winding it, and when the thickness of the insulating coating is constant?—

Answer; make

$$\frac{\text{the resistance of the } coils}{\text{the } external \text{ resistance}} = \frac{\text{the radius of the } bare \text{ wire}}{\text{the radius of the } covered \text{ wire.}}$$

(see C. Maxwell's Electricity, page 321,)
which equation, of course, reduces itself to making the resistance of the galvanometer equal to the external resistance when the covered wire is considered as having the same diameter as the bare wire.

2. What is the best shape to give to a galvanometer coil?—

Answer; make the outline of a section through the axis of the instrument satisfy the polar equation.

$$r^2 = x^2 \sin \theta$$

where r is the radius vector, θ the variable angle, and x the height of the coil measuring from the axis.

(See C. Maxwell's Electricity, page 322.)

3. If wire of *various* thicknesses be wound on the galvanometer coil, but so that the surface bounding the wire of any one thickness satisfies the preceding polar equation, then what gauges of wire should be employed?—

Answer; if y is the radius of the bare wire in a layer whose maximum distance from the centre of the coil is x, and if Y^2, a function of y, is the average space allotted to the wires in this layer including waste space between them, then the following differential equation must be fulfilled to obtain maximum sensibility.

$$\frac{x^2}{y^2}\left(1 + \frac{Y}{y}\frac{dy}{dY}\right) = \text{a constant.}$$

(See Maxwell's Electricity, page 342.)

Now if Y is proportional to y then the preceding differential equation leads to the result that both Y and y must be proportional to x, or the radius of the wire varies as the linear dimensions of the coil.

But under these circumstances what should be the resistance of the galvanometer? Should condition (1) be satisfied, that is, should the resistance of the galvanometer bear to the external resistance the ratio that the radius of the bare wire bears to that of the covered, or, if not, what is the condition that gives maximum sensibility to the galvanometer? As we do not find in books the answer to this question, we have thought it not unworthy of the notice of the Society although the method by which it is obtained is comparatively simple.

It may easily be shown that if G is the resultant magnetic force at the centre of a galvanometer coil due to unit current, and if R is the resistance of the coil, and ρ the specific resistance of the metal employed in making the wire, then

$$d\,G = N\frac{dx}{Y^2},$$

and

$$d\,R = N\frac{\rho}{\pi}\frac{x^2\,dx}{Y^2\,y^2}$$

where N is a numerical quantity. Consequently if y and Y are both proportional to x, the condition assumed above, we have, by integrating, if

$$y = a\,x$$

and
$$Y = \beta\,y$$

$$G = N\frac{1}{a^2\,\beta^2}\left(\frac{1}{a} - \frac{1}{x}\right)$$

$$R = N\frac{\rho}{\pi}\,\frac{1}{a^4\,\beta^2}\left(\frac{1}{a} - \frac{1}{x}\right)$$

where a is the radius of the cylindrical space left at the centre of the coil for the needle and mirror, and consequently not filled with wire.

Now if E is the electromotive force of the battery, and r the external resistance in circuit, the total magnetic force at the centre of the coil is

$$\frac{N}{a^2\,\beta^2}\left(\frac{1}{a} - \frac{1}{x}\right) \times \frac{E}{r + N\frac{\rho}{\pi}\,\frac{1}{a^4\,\beta^2}\left(\frac{1}{a} - \frac{1}{x}\right)}.$$

Then a, which determines the relationship between y and x, is our variable which has to be evaluated, while β is, by the conditions of the problem, a constant.

For $\frac{N}{\beta^2}\left(\frac{1}{a} - \frac{1}{x}\right)$ write M, then the expression given above becomes

$$\frac{E}{\frac{r}{M}a^2 + \frac{\rho}{\pi\,a^2}},$$

which, by the well-known laws of the differential calculus, is a maximum when

$$\frac{r}{M}a^2 = \frac{\rho}{\pi\,a^2},$$

or *when the resistance of the galvanometer is equal to the external resistance.*

Curiously enough then we arrive at the same condition for maximum sensibility both when the radius of the bare wire is equal to that of the covered (that is when the thickness of the covering is

altogether neglected) and when the area allotted to each wire bears to the area occupied by the metal itself a constant ratio in every layer.

W. E. Ayrton.
John Perry.

April, 1878.
The Imperial College of Engineering, Japan.

THE RESISTANCE OF THE ARC OF THE ELECTRIC LIGHT.

By Professors W. E. Ayrton and John Perry,

of the Imperial College of Engineering, Tokio, Japan.

Some time back Professor Perry and myself desiring to know the resistance of the arc of the electric light for the purpose of theoretically determining the best arrangement of the battery, and not being acquainted with any source from which this information could be derived, proceeded to measure it in two distinct ways.

First:—A current from sixty Grove's cells joined in series was sent through a Duboso's lamp in circuit with a small resistance composed of many yards of bare copper wire suspended in the air, and of sufficient thickness not to be heated by the passage of the strong current. The differences of potentials between the carbons and between the ends of the known resistance were then rapidly measured one after the other a number of times with a Thomson's quadrant electrometer, using the induction plate; the two differences of potential so measured being of course proportional to the resistances, since the same current traversed the whole circuit at any moment.

The result obtained was that the resistance of the electric arc varied considerably, never however exceeding 20 ohms and having a mean value of about 12 ohms.

Second:—On a subsequent occasion we measured the resistance in another way. The current from eighty Grove's cells in series

was sent through the two coils of a Latimer Clark's differential galvanometer to which we had attached a silk fibre suspension, using the method of centering described by Prof. Perry and myself in your Journal, vol. vi. page 335, 1877. One of the coils of the differential galvanometer was in circuit with the electric light and was shunted with an extremely small shunt of thick copper wire, the other coil being also shunted and in circuit with a high resistance. Almost all the current therefore passed through the electric light circuit, and balance was obtained by altering both the shunts and adjusting the resistance coils. Although the resistances of the shunts were rather too small to be easily measured with accuracy, still the resistance of the luminous arc was correctly determined by a method corresponding with that of "double weighing," that is to say, by replacing it with a resistance coil and altering this coil until balance was obtained with exactly the same adjustments of shunts &c. as were employed when the electric light was in circuit.

The resistance of the light so determined showed that it never exceeded 29 ohms, and was equal to about 20 ohms when the light was best.

Recently having occasion to use one hundred and twenty-two Grove's cells with an electric light in connection with the celebration of the official opening of the Imperial Telegraphs of Japan and the participation of that administration, for the future, in the international system of working prescribed by the St. Petersburg convention, I took the opportunity to again measure the resistance of the electric light, using the differential galvanometer method last described, and for carrying out which I have found the Latimer Clark's galvanometer with a fibre suspension very convenient. Experiments extending over an hour showed that the resistance had a maximum value of 48 ohms, and a mean value of about 30 ohms when the carbon points were a little less than three quarters of an inch asunder and the light painfully bright.

In the first two determinations of the resistance the Grove's cells employed were of the ordinary English rectangular form with flat Wedgewood porous cells, the area of the platinum plate immersed being about 13 square inches, and of the zinc plate about 25 square inches. In the third and last experiment one hundred

and eight of the cells were of the same kind, and the remaining fourteen were very large round cells, the area of each platinum plate immersed being about the same as before, but that of each zinc plate immersed being larger; on the other hand, however, the distance between the platinum and zinc in each of these larger cells was greater, so that in all cases the resistance of each cell may be regarded as having been approximately the same, or about 0·2 ohms.

The results then obtained may be shortly stated thus—

Number of Grove's cells in series.	Resistance of the battery.	Approximate mean resistance of the electric arc.
60	12 ohms	12 ohms
80	16 „	20 „
122	24·4 „	30 „

With the ordinary rectangular English Grove's cells then the resistance of the light appears to be about equal to that of the battery when sixty cells are employed, and to increase more rapidly than that of the battery as the number of cells is increased. If this law be found to be true for larger batteries, or even if the resistance of the electric light be only found to increase proportionately to that of the battery, still, contrary to what is stated in certain books, it will follow that the cells used for the production of the electric light should in every case all be joined in series and no part in parallel circuit, even when some hundreds are employed.

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ON ELECTRO-MAGNETS, &c.

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1. The following investigations have reference to the electro-magnetic induction of electro-magnets and suspended iron wires, especially as regards its influence on the speed of working. The resistance of electro-magnets to obtain the greatest magnetic force from reversals is also considered, as well as other matters which may be useful to the members of the Society.

2. Suppose we have a circuit containing a battery and an electro-magnet, and that a constant current is flowing through the circuit, which is so far removed from other circuits, &c., that there is no appreciable induction between them. If the electro-motive force is removed without breaking the circuit, say by shunting the battery, or if a new circuit is made containing the electro-magnet, the current, which has now no impressed electro-motive force to support it, nevertheless does not cease immediately, but continues to flow in the same direction with continuously decreasing strength, until it is stopped by the resistance of the circuit. We may compare the electric current under these circumstances to a material current, as of water flowing through a pipe. If it be set in motion by external force, and the latter be then removed, the water will continue to flow until it is stopped by frictional resistance. There is an exact analogy if we suppose that the water meets with a resistance exactly proportionate to its velocity. Suppose the pipe to be of unit section, M the whole mass of water in the pipe, and v its velocity, at any time t. Its momentum is Mv. Let the whole frictional resistance, which is a force acting against the stream, be Rv, proportional to the velocity. Then the equation of motion, when external force is removed, is

$$\mathrm{M}\frac{dv}{dt} = -\mathrm{R}v,$$

whence

$$v = \mathrm{V}\epsilon^{-\frac{\mathrm{R}t}{\mathrm{M}}};$$

which gives the velocity v, at time t, compared with V, the starting velocity. The total quantity that flows past every section of the pipe is

$$\frac{\mathrm{MV}}{\mathrm{R}}.$$

3. In the electric circuit the electro-motive force arising from electro-magnetic induction is proportional to the rate of decrease of the current, and to a constant depending on the form and position of the coils, cores, &c. If γ is the current at time t, and L the coefficient of self induction or electro-magnetic capacity of the circuit, and R its resistance, the equation of electro-motive force is

$$\mathrm{L}\frac{d\gamma}{dt} = -\mathrm{R}\gamma.$$

Therefore the current at time t is

$$\gamma = \Gamma\epsilon^{-\frac{\mathrm{R}t}{\mathrm{L}}},$$

where Γ is the initial current; and the integral extra current Q, or the amount of electricity that flows in the circuit after the electromotive force that produced the current in the first place is removed, is

$$\mathrm{Q} = \int_0^\infty \gamma\, dt = \frac{\mathrm{L}\Gamma}{\mathrm{R}}.$$

These equations are exactly similar to those used in the water-pipe analogy. $\mathrm{L}\Gamma$ is the electro-magnetic momentum of the circuit containing the current Γ, corresponding to MV, the momentum of the water. Also $\frac{\mathrm{MV}^2}{2}$ is the kinetic energy of the fluid, and $\frac{\mathrm{L}\Gamma^2}{2}$ the electro-kinetic energy of the current, which, however, does not reside merely in the wire, as the kinetic energy of the water is confined in the pipe, but in the surrounding space as well. The fluid by friction produces an amount of heat $= \frac{\mathrm{MV}^2}{2}$ before it is brought to rest, and the electric current produces an amount of heat $= \frac{\mathrm{L}\Gamma^2}{2}$ in the wire before it ceases. For, by Joule's law, the rate of generation of heat is $\mathrm{R}\gamma^2$, therefore the whole amount is

$$\int_0^\infty \mathrm{R}\gamma^2\, dt = \mathrm{R}\int_0^\infty \Gamma^2\epsilon^{-\frac{2\mathrm{R}t}{\mathrm{L}}}\, dt = \frac{\mathrm{L}\Gamma^2}{2}.$$

The analogy between the electric current and the flow of a material fluid, which is a very useful one, may be carried much further if required. As an example, if a pipe containing water connect two reservoirs of limited capacity, and a difference of pressure be established between them, a state of equilibrium will be arrived at through a series of oscillations of the water through the pipe. The first current from the higher level to the lower will not cease when the levels are equalized, for the water in the pipe must keep moving on till its momentum is destroyed, partly by frictional resistance and partly by the excess of pressure produced in the reservoir to which the water flows. This excess of pressure causes a reverse current to set in, and the process is repeated forwards and backwards until all the potential energy due to the original difference of level is used up, a portion being converted into the kinetic energy of heat during each oscillation, the kinetic energy of the moving water being its intermediate form. An exactly parallel case is produced by charging a condenser, *i.e.*, causing a difference of potential between the two coatings, and then discharging it through a coil. The first current, from the higher potential to the lower, as it acquires momentum, carries more than enough electricity to restore equilibrium, thus causing a reverse current, and so on. Thus there may be a series of currents, each in the opposite direction to and carrying less electricity than the preceding. The electrostatic energy of the original difference of potential is finally wholly converted into heat in the wire (if no external work has been done), a portion during each oscillation, the electro-kinetic energy of the current being its intermediate form. The analogy must not, however, be carried too far, for the starting or stopping of a material current in one pipe does not cause any current in a neighbouring pipe, as the starting or stopping of an electric current in one wire does in a neighbouring wire.

4. **Maxwell** (vol. ii.) gives the necessary information for the calculation of L from the form of the circuit, &c.; also how to measure it experimentally by comparison with the capacity of a condenser, using the Bridge arrangement. Or, it may be roughly determined by observing the integral extra-current Q. Send a

known current Γ through the electro-magnet whose electro-magnetic capacity is required, and calculate Q from the throw of the needle of a galvanometer through which the extra current is then made to flow.

Then $$L = \frac{RQ}{\Gamma}$$

where R is the resistance of the circuit through which the extra current passes, starting from the electro-magnet. The electro-magnetic capacity of the galvanometer does not affect the result, though the motion of the galvanometer magnet introduces an error. Neither will it be affected by shunting the galvanometer, whatever may be the self-induction of the shunt, or the induction, if there be any, between the galvanometer coil and the shunt, for the integral extra current will divide between the galvanometer and shunt in the inverse proportion of their resistances. Of course Q is increased by the shunt, R being at the same time equally reduced; hence it is necessary to know R. Mr. Preece, in his lecture on "Shunts," has described numerous observations of the extra currents from electro-magnets under various circumstances, but we cannot calculate L from them, even proportionately, since R is not given.

5. When an electro-motive force is introduced into a circuit, the current rises from zero to its final strength, in the same manner as it falls from it when the electro-motive force is removed and the circuit unbroken. If E is the impressed electro-motive force, as of a battery inserted in the circuit, then

$$E = R\gamma + L\frac{d\gamma}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Thus, when the current is rising, at time t, a portion of E, viz. $R\gamma$, is employed in maintaining, according to Ohm's law, the current γ already established; the other portion of E, viz., $L\frac{d\gamma}{dt}$ is employed in increasing the electro-magnetic momentum $L\gamma$. The solution of (1) is

$$\gamma = \frac{E}{R}\left(1 - \epsilon^{-\frac{Rt}{L}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The current rises in the same manner as it does in a wire con-

necting the two terminals of a condenser, which allows the determination of L by comparison with the capacity of a condenser, as before mentioned.

If the circuit be broken at any point when a current flows through it, the current does not cease quite instantaneously, but, as there is no conductive circuit, + electricity accumulates at one of the broken ends, and − at the other. The electrostatic capacity of the ends being extremely small, a high difference of potential is produced between the ends, and the dielectric breaks down, with the well-known spark as a result. This is analogous to the bursting of a pipe by the great pressure produced by the sudden stoppage of the flow of a fluid through it.

If a suspended wire, especially an iron wire, as is usual, form a part of the circuit, there may be oscillations in the current during its establishment and decay. They are due to the combined action of electro-static and electro-magnetic induction, for the wire is not only a conductor but a condenser as well, or rather one coating of a condenser. The establishment of the permanent state of the potential of the wire may take place with oscillations, and is quite a different sort of phenomenon to what occurs in a long submarine cable similarly acted upon by an electro-motive force at one end. The presence of an electro-magnet in the circuit, however, has a material influence.

6. Let there now be a simple harmonic variation of electromotive force

$$\mathrm{E} \sin mt$$

in the circuit of resistance R and electro-magnetic capacity L; then

$$\mathrm{E} \sin mt = \mathrm{R}\gamma + \mathrm{L}\frac{d\gamma}{dt}$$

where γ is the current at time t. The solution is

$$\gamma = \frac{\mathrm{E}}{\sqrt{\mathrm{R}^2 + \mathrm{L}^2 m^2}} \sin\left(mt - \tan^{-1}\frac{m\mathrm{L}}{\mathrm{R}}\right),$$

neglecting a vanishing term. Thus the amplitude of the current waves is reduced from

$$\frac{\mathrm{E}}{\mathrm{R}},$$

what it would be were there no retardation, to

$$\Gamma = \frac{E}{\sqrt{R^2 + L^2 m^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where Γ signifies the maximum strength of current.

If Lm is large compared with R, Γ is a small fraction of $\frac{E}{R}$ Also, variations in the value of R cause much smaller variations in Γ.

7. This has application to the Bell telephone. This most sensational application of electricity appears to be very indifferent to resistance, it being said to be sometimes sufficient merely to make earth through the boot and a blade of grass. Let

$$m = \frac{2\pi}{T},$$

then T is the period of a complete wave. $\frac{L}{R}$ is also a time-interval. Suppose $T = \frac{1}{1000}$ second, and $\frac{L}{R} = \frac{1}{100}$ second, then a simple calculation applied to equation (3) will show that R must be increased about 110 times to reduce the current to a half, and about 625 times to reduce the current to a tenth part.

Since m is proportional to the pitch, for sufficiently high pitches Γ is inversely proportional to the pitch. Hence it is impossible for a receiving telephone to give forth the same sounds as produced at the sending end, irrespective of mechanical and acoustical difficulties, except in the case of a single pure tone. For in any tone the second partial will be weakened twice as much as the first, or fundamental, the third three times as much, and so on; thus producing a want of brilliancy.

We do not deal in such rapid reversals when using ordinary recording telegraphs. The above value of T, viz. $\frac{1}{1000}$ second, would, with the Morse code, produce at least 2500 words per minute, or $42\frac{1}{2}$ per second, which is considerably faster than the most rapid speaker can talk. As, however, the telephone is sensible to very much more rapid reversals than 1000 per second, the enormous speeds possible on short lines is easily conceivable, could the action be sufficiently magnified and recorded, so as to appeal to the eye instead of the ear.

8. Let now R and L belong to the electro-magnet alone, and R_1 and L_1 be the resistance and electro-magnetic capacity of the remainder of the circuit. Then

$$\Gamma = \frac{E}{\sqrt{(R + R_1)^2 + m^2 (L + L_1)^2}} \quad . \quad . \quad . \quad . \quad (4)$$

Suppose the diameter of the wire of the electro-magnet to be variable. Let n be the number of turns in unit of length, or number of layers in unit of thickness. Then the magnetic force will vary as n^2, while both R and L both vary as n^4. This makes the strength of the signals capable of a maximum, dependent on the variation of n; which by (4) will be when

$$R^2 + L^2 m^2 = R_1^2 + L_1^2 m^2 \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The left side refers to the electro-magnet, the right to the remainder of the circuit. We may write (5) thus:

$$\frac{R}{R_1} = \sqrt{\frac{1 + \frac{L_1^2}{R_1^2} m^2}{1 + \frac{L^2}{R^2} m^2}}.$$

Now $\frac{L_1}{R_1}$ is constant for the same line wire, whatever its length, since both L_1 and R_1 vary as the length of the line. Also $\frac{L}{R}$ is constant for the same solenoidal coil, if only the diameter of the wire is variable, since L and R both vary as n^4. But the time-interval $\frac{L}{R}$ for the electro-magnet is in general much greater than the time-interval $\frac{L_1}{R_1}$ for the line wire, whence it follows that R must be much less than R_1 to produce the maximum magnetic force when the speed is considerably high; and the higher the speed, which is proportional to m, the smaller should the resistance of the electro-magnet be.

The calculation of $\frac{L_1}{R_1}$ is easy, as the line wire is long, straight, and parallel to the earth; but the calculation of $\frac{L}{R}$ is not so easy, owing to the variety of shapes assumed by electro-magnets used for telegraphic purposes, with their cores, pole-pieces, armatures, &c.,

which all influence the electro-magnetic capacity, though they do not influence the resistance. It is therefore impossible to enunciate a general law, that the resistance of an electro-magnet should be such or such a fraction of the external resistance to obtain the maximum effect, for the result will be different not only for different speeds, but also for different constructions of the electro-magnet.

9. Taking the case of a solenoidal electro-magnet, approximate results are easily obtainable. Let its length be l, external radius x, internal radius y, with an iron core of radius z. Its electro-magnetic capacity is

$$L = \int_y^x 4\,\pi n^2\, l dr \ \Big\{ \int_r^x n^2\, dr'\ (\pi r^2 + 4\,\pi\kappa\,\pi z^2)$$
$$+ \int_y^r n^2\, dr'\ (\pi r'^2 + 4\pi\kappa\,\pi z^2) \Big\}$$

or $\quad L = \frac{4}{3}\,\pi^2\, ln^4\,(x-y)^2\,(x^2 + 2xy + 3y^2 + 24\,\pi\kappa z^2)\ .\ \ .\ \ .\ (6)$

(Maxwell, vol. ii. p. 283), where κ is the co-efficient of magnetisation. The resistance is

$$R = \pi\rho l n^4\ (x^2 - y^2),$$

where ρ is the resistance of unit of length of wire, of unit diameter. Therefore

$$\frac{L}{R} = 16\,\pi^2\,\frac{\kappa}{\rho}\,\frac{x-y}{x+y}\,z^2 \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (7)$$

omitting $x^2 + 2xy + 3y^2$ in the expression for L, as small compared with $24\pi\kappa z^2$, which is a large number, unless the core is very small. Let $\kappa = 32$; also, if the specific resistance of copper is taken at 1·7 microhms = 1700 c. g. s., then $\rho = 1700 \times \frac{4}{\pi}$, and

$$\frac{L}{R} = 2{\cdot}33\,\frac{x-y}{x+y}\,z^2 \text{ seconds} \quad . \ . \ . \ . \ . \ . \ . \ . \quad (8)$$

10. Maxwell (vol. ii. p. 289) gives the co-efficient of self-induction of a straight wire, when the circuit is completed by a parallel straight wire. The same method of calculation is applicable to any number of straight parallel wires by finding the integral

$$T = \tfrac{1}{2} \iiint Hw\,dx\,dy\,dz,$$

where T is the kinetic energy of the system, and H, w, are the

vector-potential and the current at the point x, y, z. Thus, for parallel straight wires of length l, conveying currents C_1, C_2, C_3, . . ., of radii a_1, a_2, . . ., specific magnetic capacities μ_1, μ_2, . . .; then, representing the distance between the centres of two wires m and n by b_{mn}, we shall have

$$\begin{aligned}\frac{2\,T}{l} &= \tfrac{1}{2}\,(\mu_1\,C_1^2 + \mu_2\,C_2^2 + \ldots\ldots) \\ &\quad - 2\mu_0\,(C_1^2 \log a_1 + C_2^2 \log a_2 + \ldots\ldots) \\ &\quad - 4\mu_0\,(C_1\,C_2 \log b_{12} + C_1\,C_3 \log b_{13} + \\ &\qquad C_2\,C_3 \log b_{23} + \ldots\ldots) \quad . \quad . \quad . \quad . \quad (9)\end{aligned}$$

with the sole condition

$$0 = C_1 + C_2 + C_3 + \quad . \quad . \quad .$$

From the last two equations the co-efficients of self and mutual induction may be found. Let there be only four wires, forming two circuits, 1 and 3 for one circuit, 2 and 4 for the other; then $C_3 = -C_1$, and $C_4 = -C_2$. Substituting in (9),

$$\begin{aligned}\frac{2T}{l} &= C_1^2\left(\frac{\mu_1 + \mu_3}{2} + 2\mu_0 \log \frac{b^2_{13}}{a_1 a_3}\right) \\ &\quad + C^2_2\left(\frac{\mu_2 + \mu_4}{2} + 2\mu_0 \log \frac{b^2_{24}}{a_2 a_4}\right) \\ &\quad + 2C_1\,C_2 \times 2\mu_0 \log \frac{b_{14}\,b_{23}}{b_{13}\,b_{24}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)\end{aligned}$$

The co-efficient of C_1^2 in (10) is the co-efficient of self-induction per unit of length of the circuit conveying the current C_1. Similarly for C_2; and the co-efficient of $2\,C_1\,C_2$ is the co-efficient of mutual induction of the two circuits per unit of length.

From (10) we may find the co-efficients of self and mutual induction of two suspended wires, the circuits being completed through the earth. Let M be the co-efficient of mutual, and L_1 L_2 the co-efficients of self-induction of two wires of length l, radii a_1, a_2, heights above ground h_1, h_2, horizontal distance apart d, and specific magnetic capacities μ_1, μ_2; then

$$\left.\begin{aligned}\frac{L_1}{l} &= \frac{\mu_1}{2} + 2 \log \frac{2h_1}{a_1} \\ \frac{L_2}{l} &= \frac{\mu_2}{2} + 2 \log \frac{2h_2}{a_2} \\ \frac{M}{l} &= \log \frac{d^2 + (h_1 + h_2)^2}{d^2 + (h_1 - h_2)^2}\end{aligned}\right\} \quad . \quad . \quad . \quad (11)$$

where μ_0 has been put $= 1$.

As a special case, let

$$h_1 = h_2 = 3 \text{ metres}, \ d = \cdot 5 \text{ metre},$$
$$a_1 = a_2 = \cdot 002 \text{ metre},$$
$$\mu_1 = \mu_2 = 1 + 4\pi\kappa = 315, \text{ if } \kappa = 25,$$

then $L_1 = L_2 = 173$, and $M = 5$, approximately.

Also, if the resistance per mile is 13 ohms, the resistance per centimetre is 80778 c. g. s., therefore

$$\frac{L_1}{R_1} = \frac{173}{80778} = \cdot 00214 \text{ seconds} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

11. This time-interval being in general very small compared with $\frac{L}{R}$ for the electro-magnet, we may neglect it, and then

$$\frac{R}{R_1} = \frac{1}{\frac{Lm}{R}} \text{ approximately} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

The resistance of the electro-magnet is thus inversely as the speed to obtain the maximum strength of signals, except for low speeds. Inserting in (13) the value of $\frac{L}{R}$ given in (8),

$$\frac{R}{R_1} = \frac{T}{14{\cdot}64 \, \frac{x - y}{x + y} \, z^2},$$

where $T = \frac{2\pi}{m}$.

At 100 words per minute Morse code T = about $\frac{1}{40}$ second, therefore at this speed

$$\frac{R_1}{R} = 585{\cdot}6 \, \frac{x - y}{x + y} z^2.$$

Suppose $x = 2$, $y = z = 1$ centimetre, then

$$\frac{R_1}{R} = 195{\cdot}2,$$

or the resistance of the electro-magnet is $\frac{1}{195}$th of the external resistance to obtain the maximum magnetizing force. This is increased to $\frac{1}{174}$th when the self-induction of the suspended wire is taken into account.

12. Having made the magnetizing force a maximum for a given speed and dimensions of electro-magnet, we may next find the ratio

between the outer and inner radius of the coil to make the attractive force on a soft iron armature a maximum. We have

$$\Gamma = \frac{E}{\sqrt{(R + R_1)^2 + L^2 m^2}}$$

neglecting L_1, where

$$L = \tfrac{2}{3}\pi^2 ln^4 (x-y)^2 (x^2 + 2xy + 3y^2 + 24\pi\kappa z^2),$$

and $R = \pi\rho ln^4 (x^2 - y^2)$;

also $$F = \Gamma G,$$

where F is the magnetizing force, and

$$G = 4\pi n^2 (x - y).$$

To make F a maximum we found

$$R^2 + L^2 m^2 = R_1^2,$$

therefore $$F = \frac{EG}{R_1 \sqrt{2\left(1 + \frac{R}{R_1}\right)}}.$$

Substituting $$4\sqrt{\frac{\pi (x-y) R}{\rho l (x+y)}} \text{ for } G,$$

we have $$F^2 = \frac{\frac{8\pi (x-y) E^2}{\rho l (x+y)}}{R_1 \left(1 + \frac{R_1}{R}\right)}.$$

Now $\frac{R_1}{R} = \frac{Lm}{R}$ approximately,

$$= \frac{2m\pi}{3\rho} \frac{x-y}{x+y} (x^2 + 2xy + 3y^2 + 24\pi\kappa z^2)$$

therefore

$$F^2 = \frac{\frac{12 E^2}{R_1}}{lm (x^2 + 2xy + 3y^2 + 24\pi\kappa z^2) + \frac{3\rho l}{2\pi} \frac{x+y}{x-y}}$$

Now the magnetization of the core is proportional to the magnetizing force, and the attractive force between the core and a soft iron armature placed close to it is proportional to the square of the magnetization and to the cross section of the core. Therefore, if A is the attractive force,

$$A \propto \frac{z^2}{lm (x^2 + 2xy + 3y^2 + 24\pi\kappa z^2) + \frac{3\rho l}{2\pi} \frac{x+y}{x-y}}$$

This increases with z, so let $z = y$, the inner radius of the coil. Let x be constant and y variable, then A is a maximum when

$$\frac{x^2}{y^2} + \frac{2x}{y} + \frac{3\rho}{2\pi y^2 m} \frac{x+y}{x-y}$$

is a minimum; *i.e.*, when

$$\frac{2\pi m}{3\rho} x^2 = \frac{\frac{y}{x} - \left(1 - \frac{y^2}{x^2}\right)}{\left(1 - \frac{y}{x}\right)\left(1 - \frac{y^2}{x^2}\right)}$$

The least value of $\frac{y}{x}$ is

$$\frac{y}{x} = \frac{\sqrt{5} - 1}{2} = \cdot 618$$

Using the former value of ρ, viz., $1700 \times \frac{4}{\pi}$; also $m = 80\pi$, and $x = 2$ centimetres,

$$\frac{y}{x} = \cdot 7 \text{ nearly},$$

$\frac{y}{x}$ increases very slowly as x and m increase.

If y be constant and x variable, smaller values of $\frac{y}{x}$ are obtained.

The attractive force also varies inversely as the length of the coil; that is to say, if the length of the coil is halved, preserving its other dimensions, as well as its resistance constant, the attractive force is doubled. Although this result is only true for long coils, it points in the direction of short coils being the best, especially as the attractive force is increased by increasing the transverse dimensions for any fixed ratio of $\frac{y}{x}$. We have, however, neglected the increase in the self-induction due to the armature.

13. In the determination of the resistance of an electro-magnet in paragraph 11 and before, only one electro-magnet is considered to be in the circuit. The results are inapplicable when there is another in circuit, used for sending the signals for example. Let two similar electro-magnets, each of resistance R and electro-magnetic capacity L, be used telephonically, on Bell's principle. Let E sin mt be the electro-motive force induced in the sending electro-

magnet, then the maximum strength of the currents in the circuit is, by equation (3),

$$\Gamma = \frac{E}{\sqrt{(2\,R + R_1)^2 + 4\,L^2\,m^2}}$$

where R_1 is the line-resistance. Let F be the magnetizing force in the receiving coil, then

$$F = \frac{EG}{\sqrt{(2\,R + R_1)^2 + 4\,L^2\,m^2}}$$

where G is the magnetising force of the unit current. Now, suppose the thickness of the wire in both coils variable together, then E and G both vary as the number of turns, *i.e.*, as n^2, while R and L both vary as n^4. Let $G = n^2 g$, $E = n^2 e$, $R = n^4 r$, $L = n^4 l$, then

$$= \frac{eg}{\sqrt{\left(2r + \frac{R_1}{n^4}\right)^2 + 4l^2\,m^2}}$$

whence it may be seen that F increases with n, solely by the reduction of the term $\frac{R_1}{n^4}$; but the increase is very slow after passing certain limits, as will be seen from the following example. Let $R_1 = 1000$ ohms, $\frac{L}{R} = \frac{1}{10}$, $m = 1200$. Then for the following values of R, viz., $\frac{10}{16}$, 10, 160, ∞, ohms, we have the following proportional value of F, $\frac{1}{16200}$, $\frac{1}{2600}$, $\frac{1}{2401}$, $\frac{1}{2400}$. Thus under 10 ohms the increase is rapid; after that, next to nothing.

14. Induction from wire to wire. Two suspended circuits, A and B, have resistances R_1 and R_2, electro-magnetic capacities L_1 and L_2, mutual capacity M. If reversals are made by an electro-motive force E sin mt in the primary circuit A, and there is no electro-motive force in B except that induced by changes of the current in A, then

$$E \sin mt = \left(R_1 + L_1 \frac{d}{dt}\right) \gamma_1 + M \frac{d\gamma_2}{dt},$$

$$0 = \left(R_2 + L_2 \frac{d}{dt}\right) \gamma_2 + M \frac{d\gamma_1}{dt};$$

where γ_1 and γ_2 are the currents in A and B. From these we shall find, if Γ_1 and Γ_2 are the amplitudes of the currents in A and B,

$$\Gamma_1 = \frac{E\sqrt{R_2^2 + L_2^2 m^2}}{\sqrt{\{R_1 R_2 - m^2(L_1 L_2 - M^2)\}^2 + m^2(R_1 L_2 + R_2 L_1)^2}}$$

and
$$\frac{\Gamma_2}{\Gamma_1} = \frac{Mm}{\sqrt{R_2^2 + L_2^2 m^2}}$$

This ratio cannot be greater than $\frac{M}{L_2}$. If the wires are as in the special case of paragraph 10, $M = 5$, and $L_2 = 173$; therefore $\frac{M}{L_2} = \frac{1}{34}$, expresses the greatest value of the ratio of the induced to the inducing currents.

Let each of the lines A and B be 10 miles in length, of resistance 130 ohms, and let the secondary circuit have an electro-magnet at each end of resistance 65 ohms. If $L_2 = L'_2 + L''_2$, where L'_2 belongs to the electro-magnets, and L''_2 to the line, then

$$L''_2 = 173 \times 160{,}934 \times 10 = 278{,}415{,}820,$$

since there are 160,934 centimetres in a mile. And

$$M = 5 \times 160{,}934 \times 10 = 8{,}046{,}700$$

Therefore
$$\frac{\Gamma_2}{\Gamma_1} = \frac{M}{L'_2 + L''_2} = \frac{8{,}046{,}700}{L'_2 + 278{,}415{,}820}$$

If, further, the time-constant for each coil $= \frac{1}{10}$ second,

$$L'_2 = \tfrac{1}{10} \times 65 \times 10^9 \times 2 = 130 \times 10^8$$

and
$$\frac{\Gamma_2}{\Gamma_1} = \frac{8}{13278} = \frac{1}{1659}.$$

If the time-constant is as small as $\frac{1}{100}$ second, then

$$L'_2 = 130 \times 10^7$$

and
$$\frac{\Gamma_2}{\Gamma_1} = \frac{8}{1578} = \frac{1}{197}.$$

Increasing the length of the lines, or of the portions in proximity, increases the ratio of the induced to the inducing currents; and decreasing the electro-magnetic capacity of the instruments in the secondary circuits does the same. Sudden changes in the primary current of course cause greater induced currents.

15. If a condenser be discharged through more than one circuit

simultaneously, in what manner does electro-magnetic induction affect the division of the charge? Suppose we have a condenser of capacity c, charged to potential E with a charge $Q = Ec$, and that the condenser is discharged through any number of circuits in parallel arc, of resistances R_1, R_2, ..., co-efficients of self-induction L_1, L_2, ..., and of mutual induction M_{12}, M_{23}, M_{13} . . . . If v is the potential of the condenser at time t after the commencement of the discharge, we have a system of equations equal to the number of the circuits: viz.

$$v = R_1\gamma_1 + \frac{d}{dt}\left(L_1\gamma_1 + M_{12}\gamma_2 + M_{13}\gamma_3 + M_{14}\gamma_4 + \quad . \quad . \quad\right)$$
$$v = R_2\gamma_2 + \frac{d}{dt}\left(L_2\gamma_2 + M_{12}\gamma_1 + M_{23}\gamma_3 + M_{24}\gamma_4 + \quad . \quad . \quad\right)$$
$$v = R_3\gamma_3 + \frac{d}{dt}\left(L_3\gamma_3 + M_{13}\gamma_1 + M_{23}\gamma_2 + M_{34}\gamma_4 + \quad . \quad . \quad\right)$$

&c.

when $\gamma_1, \gamma_2, \gamma_3$ . . . are the currents at time t in R_1, R_2, R_3 . . .

Integrating both sides of all the equations with respect to t between the limits $t = 0$ and $t = \infty$, we have

$$\int_0^\infty v dt = R_1 Q_1 = R_2 Q_2 = R_3 Q_3 = \ldots .$$

where $\quad Q_1 = \int_0^\infty \gamma_1\,dt, \quad Q_2 = \int_0^\infty \gamma_2\,dt, \ldots .$

since the currents are zero both for $t=0$ and $t=\infty$. It follows that Q_1, Q_2, ..., which are the integral currents through R_1, R_2, ..., are inversely proportional to the resistance, or that the total charge Q divides between the circuits in the inverse proportion of their resistance. This only applies to the whole current, for at any particular moment the currents in the different circuits do not bear the same proportion. In fact, the current in one circuit may be from, and in another to, the condenser at a certain time. The current in any circuit at any time may be calculated by finding the roots (all negative, or imaginary with real parts negative), of an algebraical equation of the $(n+1)^{\text{th}}$ degree, n being the number of circuits. The equation needed to be added to the above system of n equations is

$$-c\frac{dv}{dt} = \gamma_1 + \gamma_2 + \gamma_3 + \ldots .$$

or the current leaving the condenser = sum of currents in the wires.

In a similar manner it may be shown that if, instead of the charge of a condenser, the extra current of a coil be discharged through any number of parallel circuits, the total quantity passing through any circuit will be inversely proportional to its resistance.

16. As a special case, suppose the charge Q of a condenser of capacity c is discharged through a single coil of resistance R and electromagnetic capacity L. Then, v being the potential of the condenser and γ the current in R at time t,

$$v = R\gamma + L\frac{d\gamma}{dt},$$

$$-c\frac{dv}{dt} = \gamma,$$

whence
$$0 = v + Rc\frac{dv}{dt} + Lc\frac{d^2v}{dt^2},$$

$$0 = \gamma + Rc\frac{d\gamma}{dt} + Lc\frac{d^2\gamma}{dt^2};$$

therefore
$$\gamma = A\epsilon^{at} + B\epsilon^{bt}$$

where A and B are constants, and a and b are the roots of

$$Lcx^2 + Rcx + 1 = 0,$$

or
$$x = -\frac{R}{2L} + \sqrt{\frac{R^2}{4L^2} - \frac{1}{Lc}},$$

therefore
$$\gamma = \epsilon^{-\frac{Rt}{2L}}(A\sin + B\cos)\, t\sqrt{\frac{1}{Lc} - \frac{R^2}{4L^2}}.$$

Now when $t = 0$, $\gamma = 0$, therefore $B = 0$; and when $t = 0$, $E = L\frac{d\gamma}{dt}$, E being the initial potential of the condenser; therefore

$$A = \frac{E}{\sqrt{\frac{L}{c} - \frac{R^2}{4}}}$$

and
$$\gamma = \frac{E}{\sqrt{\frac{L}{c} - \frac{R^2}{4}}}\epsilon^{-\frac{Rt}{2L}}\sin t\sqrt{\frac{1}{Lc} - \frac{R^2}{4L^2}}$$

Let $\frac{L}{R} = \alpha$, and $Rc = \beta$, both time-intervals, then

$$\gamma = \frac{E}{R} \quad \frac{E^{-\frac{t}{2\alpha}}}{\sqrt{\frac{\alpha}{\beta} - \frac{1}{4}}} \sin \frac{t}{2\alpha}\sqrt{\frac{4\alpha}{\beta} - 1}$$

which may be put in the exponential form if $\frac{4\alpha}{\beta} < 1$. In the latter case the discharge is continuously in one direction, but if $\frac{4\alpha}{\beta} < 1$, the discharge is oscillatory.

Let the condenser have a capacity of 1 microfarad $= 10^{-15}$ c. g. s. and $\frac{L}{R} = 10^{-2}$, then

$$\frac{4\alpha}{\beta} = \frac{4L}{R^2 c} = \frac{4 \times 10^{-2}}{r \times 10^{-6}} = \frac{4 \times 10^4}{r},$$

if r is the number of ohms in R, since the ohm $= 10^9$ c. g. s. Thus the discharge is oscillatory if R is less than 40,000 ohms, and continuous if it is greater than that amount.

Suppose $R = 100$ ohms, then $\frac{4\alpha}{\beta} = 400$, and

$$\gamma = \frac{E}{R} \cdot \frac{2\epsilon^{-50t}}{\sqrt{399}} \sin 50t\sqrt{399}$$

$$= \frac{E}{10R}\epsilon^{-50t} \sin 1000t, \text{ approximately.}$$

The period of an oscillation is $\frac{2\pi}{1000}$ second $= \cdot 006$ sec. The quantity in the first current is $Q\left(1 + \epsilon^{-\frac{\pi}{20}}\right)$, a little less than twice the original charge. In the next current (in the reverse direction) it is a little less again. The total current, irrespective of its direction, is

$$\frac{Q}{1 - \epsilon^{-\frac{\pi}{\sqrt{\frac{4\alpha}{\beta} - 1}}}} = \frac{Q}{1 - \epsilon^{-\frac{\pi}{20}}} \text{ nearly.}$$

The discharge is practically over in $\frac{1}{35}$ second.

If the coil is shunted by a coil of resistance S, and no self-induction, other things being the same as before, the current γ at time t in the first coil will be found to be

$$\gamma = \frac{2\,E\,\alpha'}{L\,\sqrt{4\,\frac{\alpha'}{\beta'}-1}}\,\epsilon^{-\frac{t}{2\,\alpha'}}\quad \sin\frac{t}{2\,\alpha}\sqrt{\frac{4\,\alpha'}{\beta'}-1}$$

where

$$\alpha' = \frac{L}{R+\frac{L}{S\,c}},\qquad \beta' = \frac{c\,R+\frac{L}{S}}{1+\frac{R}{S}}.$$

Although the total currents through the coil and its shunt are in the inverse proportion of their resistances, yet the same is not true of the heat produced in the wires. The energy converted into heat in the coil R is,

$$\int_0^\infty R\,\gamma^2\,dt = \frac{E\,c^2}{2}\cdot\frac{S}{R+S}\cdot\frac{R\,Sc}{R\,Sc+L}.$$

Here $\frac{E\,c^2}{2}$ is the electrostatic energy of the original charge, and $\frac{S}{R+S}$ the shunt factor. Since the remaining factor is less than unity, it follows that the amount of heat produced in the wire R is always less than in the inverse proportion of the resistances.

17. Let us next examine the influence of a fault on rapid reversals on a land-line. Let R be the resistance from one end of the circuit up to the fault, L′ its electromagnetic capacity; let R′ and L′ be similar quantities for the other section of the circuit, and S the resistance of the fault itself. Let γ, γ', and γ'' be the currents in R, R′, and S; and let E sin mt be the electromotive force in R, and v the potential of the wire at the fault. Then

$$\left.\begin{aligned} E\sin mt - v &= \left(R + L\frac{d}{dt}\right)\gamma \\ v &= \left(R' + L'\frac{d}{dt}\right)\gamma' \\ v &= S\gamma'' \\ \gamma' + \gamma'' &= \gamma \end{aligned}\right\}$$

by the conditions of the problem; from which, for the current in R′, we have

$$E \sin mt = \left(R + R' + \frac{RR'}{S}\right)\gamma' + \left(L + L' + \frac{LR' + L'R}{S}\right)\frac{d\gamma'}{dt} + \frac{LL'}{S}\frac{d^2\gamma'}{dt^2};$$

whence the amplitude Γ′ of the waves in R′ is

$$\Gamma' = \frac{E}{\sqrt{\left(R + R' + \frac{RR' - LL'm^2}{S}\right)^2 + m^2\left(L + L' + \frac{LR' + L'R}{S}\right)^2}}$$

To find the effect of the fault, we may compare this expression with its value when $S = \infty$, or no fault.

1st Case.—Electro-magnet at one end only. $R = R'$, or the fault in the middle of the circuit, $L = 0$. Then

$$\Gamma' = \frac{E}{\sqrt{\left(2R + \frac{R^2}{S}\right)^2 + m^2 L'^2\left(1 + \frac{R}{S}\right)^2}}$$

Let $S = \frac{R}{2}$. This would reduce the strength of the current received in R′ from a constant electromotive force in R to one-half. In the above expression, however, the change of S from ∞ to $\frac{R}{2}$ doubles $\left(2R + \frac{R^2}{S}\right)$ and trebles $m\ L'\left(1 + \frac{R}{S}\right)$: so that the fault weakens the strength of rapid reversals much more than it weakens a permanent current.

2nd Case.—Electro-magnet at each end. $R = R'$, $L = L'$, and $\frac{Lm}{R} = n$.

$$\Gamma' = \frac{E}{\sqrt{1 + n^2}\sqrt{4\left(1 + \frac{R}{S}\right) + \frac{R^2}{S^2}\left(1 + n^2\right)}}$$

When there is no fault, or $S = \infty$,

$$\Gamma' = \frac{E}{2R\sqrt{1 + n^2}},$$

and when $S = \frac{R}{2}$,

$$\Gamma' = \frac{E}{2R\sqrt{1+n^2} \quad \sqrt{4+n^2}}.$$

Thus the fault reduces the current received in R′ to less than an $\frac{1}{n}$th part, and for high speeds n may be a large number, whereas a permanent current is only reduced to one-half. This applies to a telephonic circuit with a fault in the middle, and the result shows that leakage has a most prejudicial effect. We may also conclude that circuits worked by magneto-electric transmitters are more affected by leakage than when worked in a similar manner from a battery.

18. Influence of shunt on retardation. Suppose the receiving instrument has resistance R and electromagnetic capacity L, shunted by a coil of resistance S and capacity L′. Let the line resistance be A. First let there be a constant E. M. F. in A. The shunt reduces the strength of the final current in R

$$\text{from} \quad \frac{E}{A+R} \quad \text{to} \quad \frac{E}{A+\dfrac{RS}{R+S}} \times \frac{S}{R+S} = C_1, \text{ say.}$$

At the same time the shunt alters the manner in which the current rises in the electromagnet. If the shunt has no self-induction, or $L' = 0$, the current γ in R rises according to the equation

$$\gamma = C_1\left(1 - \epsilon^{-\frac{t}{\alpha}}\right)$$

where

$$\alpha = \frac{L}{R + \dfrac{AS}{A+S}}$$

The time the current takes to reach any stated fraction of its final strength is proportional to α. This time-interval is increased by the shunt of no capacity from

$$\frac{L}{R+A} \quad \text{to} \quad \frac{L}{R+\dfrac{AS}{A+S}}.$$

While the current is rising in R, it is falling in S from the strength

$$\frac{E}{A+S},$$

which is almost instantaneously reached, to

$$\frac{E}{A + \frac{RS}{R+S}} \times \frac{R}{R+S}$$

its final strength. When the electromotive force is removed, the end of the line being put to earth, the currents in R and S fall to zero in a similar manner, *i. e.* in R the current is continuously in the same direction as at first; whereas in the shunt it is immediately reversed. The integral extra current in R is

$$C_1 \alpha = \frac{ESL\,(A+S)}{(AR+RS+AS)^2}.$$

This is greatest, as depending on the resistance of the shunt, when

$$R = \frac{AS}{A+S},$$

or when the resistance of the electromagnet = resistance external to it. Since, when the current is falling in the electromagnet it draws electricity through the line and the shunt, the potential of the line is negative,

$$= -\frac{x}{l} \times \text{current in line},$$

at any point distant x from the beginning of the line of length l. Similarly for the shunt.

Now let the shunt have electromagnetic capacity L′. The differential equation for the current γ in R is

$$ES = \gamma(RA+SA+RS) + \frac{d\gamma}{dt}\left\{L\,(A+S) + L'(A+R)\right\} + \frac{d^2\gamma}{dt^2}\,LL'$$

For simplicity let A=R=S. Then, when the shunt is not on, the extra current in R is

$$\gamma = \frac{E}{2A}\,\epsilon^{-\frac{t}{\alpha_1}}, \quad \text{where } \alpha_1 = \frac{L}{2A}.$$

With the shunt on, of no self induction,

$$\gamma = \frac{E}{3A}\,\epsilon^{-\frac{t}{\alpha_2}}, \quad \text{where } \alpha_2 = \frac{2L}{3A}.$$

Thus α is increased in the ratio $\frac{1}{2}$ to $\frac{2}{3}$, or 3 : 4. Now let $L' = \frac{1}{2}L$, then

$$\gamma = \frac{E}{3A}\,\epsilon^{-\frac{3At}{L}}\left(\tfrac{1}{2}\,\epsilon^{\frac{\sqrt{3}At}{L}} + \tfrac{1}{2}\,\epsilon^{-\frac{\sqrt{3}At}{L}}\right);$$

and, since the bracketted expression is > 1, $\alpha > \frac{L}{3\,A}$.

When $L' = L$,

$$\gamma = \frac{E}{3\,A}\epsilon^{-\frac{t}{\alpha_3}} \text{ where } \alpha_3 = \tfrac{1}{3}\frac{L}{A},$$

and the current rises in the line in the same manner as it would if for the electromagnet and shunt were substituted a single coil of resistance $\frac{A}{2}$ and magnetic capacity $\frac{L}{2}$. At the same time the shunt reduces the retardation from $\alpha_2 = \frac{2\,L}{3\,A}$ to $\alpha_3 = \frac{L}{3\,A}$, or as $2 : 1$, as L' increases from 0 to L. But since $\alpha = \frac{L}{2\,A}$ when there is no shunt at all, the shunt of equal capacity to the receiver's only reduces the retardation in the ratio $3 : 2$.

$L' = 2\,L$. Here the extra current in the receiver is

$$\gamma = \frac{E}{3\,A}\,\epsilon^{-\frac{3\,At}{2\,L}}\left\{\frac{\sqrt{3}+1}{2}\,\epsilon^{-\frac{\sqrt{3}\,At}{2\,L}} - \frac{\sqrt{3}-1}{2}\,\epsilon^{\frac{\sqrt{3}\,At}{2\,L}}\right\}$$

This becomes zero when

$$t = \frac{L}{\sqrt{3}A}\log\left(2 + \sqrt{3}\right)$$

and a minimum negative when

$$t = \frac{L}{\sqrt{3}A}\log\left(7 + 4\sqrt{3}\right)$$

Thus when the shunt has a greater capacity than the receiver, when the current is put on the current in the receiver first rises above, and then falls to its final strength. When the battery is removed and earth put on at the sending end, the current in the receiver falls through zero, becomes reversed, and then rises to zero again. But we cannot exalt the current from an electro-magnetic shunt so as to send back a current *to the line* immediately after each signal, as has been stated. When, as above, the extra current in the receiver becomes reversed in direction, this reverse current does not go to line, but goes round by the shunt.

Joining the two coils of a relay in parallel arc has the effect of quartering the resistance and quartering the capacity of the relay considered as a whole; or rather, it would be so if the coils were

at a distance apart instead of being close together with the cores connected by an armature, which lessens the reduction in the retardation. But if, instead of joining the coils in parallel to reduce the retardation, we wind the coils with thicker wire, we get much more advantageous results.

19. With the same notation, let us examine the influence of the shunt on rapid reversals. Suppose the E. M. F. in A is E sin mt, then the amplitude of the currents in the electromagnet R is

$$\Gamma = \frac{E\sqrt{(L'm)^2 + S^2}}{\sqrt{\{(AR + RS + AS) - m^2 LL'\}^2 + m^2\{A(L+L') + RL' + SL\}^2}}$$

Let R=S=A, then

$$\Gamma = \frac{E\sqrt{A^2 + L'^2 m^2}}{\sqrt{(3A^2 - m^2 LL')^2 + 4m^2 A^2 (L + L')^2}}$$

First, with no shunt at all, or $S = \infty$, and R=A,

$$\Gamma = \frac{E}{A\sqrt{n^2 + 4}}$$

where $n = \frac{Lm}{A}$. Now put on shunt without capacity, S = A, L = 0, and

$$\Gamma = \frac{E}{2A\sqrt{n^2 + \frac{9}{4}}}.$$

The current is thus reduced about a half. Next let the shunt be a similar coil to the receiver, then R = S = A, L′ = L, and

$$\Gamma = \frac{E}{A\sqrt{n^2 + 9}},$$

nearly the same as without any shunt. Thus the strength of the currents in the receiver is scarcely affected by putting on a similar electromagnet as a shunt, while the initial retardation is reduced in the ratio 3 : 2, as we found in the last paragraph. Further increase of L′ has little influence on the magnitude of the signals.

NOTES ON SOME TELEGRAPH LINES LATELY CONSTRUCTED FOR THE PERSIAN TELEGRAPH ADMINISTRATION.

By A. Houtum Schindler. M.S.T.E.

Teheran, 1st June, 1878.

On the 1st December, 1877, was completed the line from Hamadan to Sinendrij (also called Sinna), the capital of Persian Kurdistan. It has a length of 123 kilomètres; its poles are poplar and platanus or plane; it has two stations, Qorveh and Sinna. The poles had been put up in September, the wire arrived late and was put up in November, when (Kurdistan lies on an average 5,000 feet above the level of the sea) deep snow was on the ground.

On the 7th April, 1878, was completed the line from Burújird to Shúshter, with a length of 340 kilomètres and seven new stations: Rázán, Khorremábád, Násserábád, Muktberábád, Rezzeh, Dizfúl, and Shúshter. The construction of this line presented more than ordinary difficulties; a distance of 132 miles between Khorremábád and Dizfúl is totally devoid of fixed habitations, and is made very unsafe by the nomad Lur tribes. Three stations, little fortresses, were built, the tribes were made friends with, labourers were paid double wages, and strict guard was always kept. Since the completion of the line, with the exception of a few days during which a tribe pulled the line down and stole about 400 insulators, no interruption has taken place. For this line I had poplars, oaks, planes, and a kind of willow.

The third line is a little one of 39 kilomètres from Burújird to Nehávend. It was completed the 28th May.

For all the lines the insulators have been made in Persia; they did very well, but were in some instances not strong enough. The wire was Messrs. Johnson Nephews' No. 9 galvanised, and the best we ever had in Persia; the instruments were Siemens' Morse, the batteries Minotto and Daniell.

A. Houtum Schindler,
Inspector-General of Persian Telegraphs.

CORRESPONDENCE.

To the Secretary of the Society of Telegraph Engineers.

Sir,

The following experiments carried out by me in December last appear to demonstrate that a solid conductor heated by a current of electricity offers a less electrical resistance than when heated to the same temperature by other and external means.

Table showing the Electrical Resistance in ohms of one foot of Iridio-Platinum Wire (about 10 per cent. Iridium) 0·003 inch diameter.

Number of Piece.	Cold, say 15° C.	Brilliant white heat, say 1500° C.		Remarks.
		By Gas Furnace.	By Electric Current.	
1	15·46	39·84	34·20	Wire in each case just below the point of fusion; a small increase in the gas or electric current producing fusion.
2	15·78	40·18	34·50	

The manner in which this was done was as follows:

The resistance of one foot of the wire was balanced by the Wheatstone's Bridge arrangement; such a testing current being used that one-half of it was sufficient to bring the wire to a white heat. With this end in view, three sets of resistance coils each containing 100 ohms, and specially made for testing with the large currents used for firing the fine wire fuzes in submarine mines, were employed to form three sides of the balance, the fourth being occupied by one foot of the standard wire hung between brass clips on standards. The coils used were specially made so that

strong currents of electricity do not produce appreciable alterations in the resistance, and care was taken to make short contacts.

The resistance of the foot of wire was first found with two cells, the temperature being about 15° C.; the battery was then increased to 100 cells, each having an internal resistance of about 0·2 ohm and an E. M. F. of about 1·5 volt. A rough detector was first used as the galvanometer, and, after a few trials with snapping contacts, it was found that, with 40 ohms open in each leg of the balance, the wire in the fourth leg was balanced, no current passing through the detector but a current passing through the wire sufficient to bring it to a white heat.

The detector was then replaced by a delicate galvanometer and the wire balanced at a white heat with a longer contact, within $\frac{1}{20}$ ohm, the set of coils being subdivided to read to twentieths.

A second foot of wire was then taken from the same spool, and similarly treated, with the results shown on the table.

I have recently been informed that the results of some experiments, made in order accurately to discover the internal resistance of a Grove's cell of given size, have pointed to the conclusion that this resistance alters, within certain limits, with the external resistance in circuit. In other words, that the greater the current the less is the liquid resistance (within certain limits) of the cell or battery producing the current.

This result appears to harmonise with the facts already tabulated, and to indicate that the same law applies both to liquid and to solid conductors: viz. that a current of electricity has a tendency to arrange the molecules of all conductors along which it is passing, in a manner so that the conductivity of the conductor is increased.

This action is doubtless analogous to that which obtains in an insulating medium when an increase of the electromotive force applied produces a decrease in the resistance of the dielectric.

I am, &c.,

J. T. Bucknill,

Capt. R.E.

Horse Guards,

5 *August*, 1878.

AMROLI, INDIA, *4th September*, 1878.

THE SECRETARY THE SOCIETY OF TELEGRAPH ENGINEERS.

SIR,

I have the honour to submit some remarks on the effects of a distant thunderstorm as noticed by me whilst using a telephone joined in circuit with a microphone, 6-cell Leclanché battery, No. 16, B. W. G. copper-wire and the earth.

The microphone is placed in my workshop, and the telephone in my bungalow, a distance of some 200 yards away; the microphone being placed on the same shelf as the workshop clock, in order that I might show my friends its wonderful sensitiveness—the ticking of the clock being perfectly audible. I can readily distinguish the sounds of filing, hammering, turning, &c. and thus form a pretty good idea of what is going on in the works. I merely mention these facts to show that the microphone and telephone are fairly sensitive ones, which may perhaps have some relation to the effects noticed by me.

All day on the 3rd of September this neighbourhood was within the radius of an electric storm, and I noticed, when applying my ear to the telephone, peculiar sharp snapping sounds which were unusual. I did not attribute them to their right cause till about 9 p.m., when it struck me that the atmospheric electricity must be answerable for the peculiar noises. This I proved beyond all doubt by experiments lasting over an hour. The lightning was very distant, and I could not hear any thunder: but I noticed that every lighting up of the sky was accompanied by a simultaneous crack in the telephone; the more vivid the flashes, the sharper the crack. There was no appreciable difference in the time between my seeing and hearing the flash. This fact may offer some interest on the relation of the speeds of electricity and light. It has struck me that a short circuit such as I mention, placed in connection with observatories, might afford some reliable information as to the amount of atmospheric electricity in the neighbourhood, and approaching storms could be foretold. The instruments used were made by native workmen.

If you think the above worthy of the notice of the Society, I shall esteem it a favour if you will bring it to their notice.

I have, &c.,

A. R. M. SIMKINS,

Associate of the Society of Telegraph Engineers.

THE WEST INDIA AND PANAMA TELEGRAPH COMPANY, LIMITED,
GENERAL SUPERINTENDENT'S OFFICE,
ST. THOMAS, W.I., 31*st May*, 1878.

TO THE SECRETARY SOCIETY OF TELEGRAPH ENGINEERS, LONDON.

SIR,

The following may not prove uninteresting to the readers of your Journal.

A severe thunderstorm passed over Colon, Isthmus of Panama, on the afternoon of the 14th of May.

The lightning struck the top of the lighthouse, an iron stay building, about eighty feet above the level of the sea, well insulated at the base by a granite foundation, alongside of which a corrugated iron cable hut is erected in which the end of the cable to Jamaica is brought in. The lightning, on arriving at the base of the lighthouse, and meeting with the resistance offered by the granite foundation, appears to have dashed across a double-cup porcelain insulator attached to one of the stays of the lighthouse forming the terminus of the line wire, and then entered the cable hut by means of a No. 16 copper wire "lead" connected to one of Bright's lightning arresters, fusing not only the leading wire but also the whole of the platinum wires and connections of the arrester in its rapid course to earth. The cable was in no way damaged, thus proving the efficiency of these lightning arresters. In order to prevent a similar occurrence we have put the lighthouse to earth by means of a lead attached to one of its iron stays, connecting with a copper earth-plate buried in the sea. I inclose a sketch of the lighthouse and cable hut connections showing the course of the electric fluid.*

I am, SIR, yours faithfully,

R. T. BROWN,

General Supt.

* The sketch can be seen at the Library of the Society.

Lightning and Railway Trains.

Mr. G. G. Newman of the Telegraph Department, London and North Western Railway, Euston Station, believing that a record of cases of trains in transit being struck by lightning would be of great interest, is desirous of collecting information on the subject, and would feel much obliged to Members who would kindly communicate to him any instances that may have come within their knowledge of trains being struck by lightning.

The Common String Telephone.

Referring to the string telephone, consisting of a piece of ordinary string stretched between two cylinders (such as the covers of cylindrical match-boxes), which for some years past has been a common plaything, not only in Europe and America, but even in India, Mr. A. Houtum-Schindler, Inspector-General of Persian Telegraphs, in a recent letter to the Secretary, mentions that in Teheran the use of this toy in the streets was carried on to such an extent as to become a nuisance, and had to be eventually prohibited by the police. After some experiments had been made in Teheran a short time ago with the Electric Telephone, the Persians remarked that their children's invention had been copied by the Europeans, and been named by them the "Telephone."

ABSTRACTS AND EXTRACTS.

ON THE BIRMINGHAM WIRE-GAUGE.

By Latimer Clark, Mem. Inst. C.E.

A Paper read at the British Association for the Advancement of Science, at Dundee, September, 1867.

The Birmingham Wire-gauge is a scale of numbers extensively employed both in this country and abroad to designate a set of arbitrary sizes of wire, varying from about half-an-inch down to the smallest sizes usually drawn.

The origin of this system and the date when it was introduced appear to be unknown, and, as there is no authorised standard in existence, it happens that a great number of gauges have come into extensive practical use under the common name of the Birmingham Wire-gauge, but which differ from each other to a most serious extent.

A Table is annexed containing the sizes, in decimal parts of an inch, of thirteen different wire-gauges collected from various sources, and it is probable that many others might be found. It will be seen from this Table that the difference in weight of wires of the same denomination, according to different Tables, amounts frequently to 10, 15, and in some cases even to 90 per cent.; and this between gauges which are in extensive daily use. The author has in fact recently been concerned in a contract in which the choice of one or other of two so-called Birmingham Wire-gauges, in equally common use and of equal authenticity, would make a difference of £8,000 to the contractor.

This discrepancy of gauges has not escaped the attention of practical men. Mr. Holtzapffel, in 1846, called attention to the uncertainty and inaccuracy prevailing among the gauges used in different manufactories; and, in 1857, Mr. Joseph Whitworth, at a meeting of the Institution of Mechanical Engineers of Manchester, proposed the adoption of a wire-gauge in which the number of each size corresponded to its diameter in thousandths of an inch—for example, a wire ·095 inch in diameter would be

called No. 95, and so on. Could this have been introduced into practical use it would have met every practical objection.

In 1858 Mr. James Cocker, of Liverpool, proposed to introduce a new Birmingham Wire-gauge, closely approximating to the gauges in ordinary use, but with some of the more glaring irregularities smoothed over, so as to render the gauge more regular in its gradations. He also introduced a very convenient instrument, which bears his name, for measuring the sizes of wire in thousandths of an inch, and which has proved very useful to practical men. He also, I believe, first proposed to employ the word "mil" as signifying the thousandth part of an inch, an innovation which is found of extreme convenience in practice.

Before making any suggestion for obviating the inconvenience arising from this great confusion among the gauges in common use, I would remark that I agree with others in entirely approving of the system of measurement in decimal fractions of an inch, and I employ this system in my own practice. In specifying sizes of wire, I describe the diameter in mils or thousandths of an inch, thereby preventing any misconception or ambiguity. It is not, however, to be expected that shopkeepers or ordinary workmen would resort to such refinements; and there is, moreover, so much practical convenience in the use of a limited number of sizes which become familiarised to the memory and to the eye, and the system is in such universal adoption throughout the world, that I see no hope or prospect of its abolition.

Under these circumstances it would appear desirable to bring into use one standard wire-gauge approximating as closely as possible to the present wire-gauges, and sufficiently so to render it possible to make the present numbers still available, but formed on a systematic plan, and introduced with sufficient authority to ensure its universal reception.

I am of opinion that if the British Association were to appoint a Committee to investigate and report upon the subject, and were to issue a gauge under their authority, bearing the title of the British Association Gauge or British Gauge, they would confer a great boon on the commercial community, and that the gauge recommended by them would soon obtain universal adoption.

I have been unable to ascertain which of the gauges given in the subjoined Table is entitled to the greatest authority, but I am inclined to attribute the greatest weight to that given in page 1013 of Mr. Holtzapffel's book on Turning, both on account of his well-known mechanical accuracy and the early date at which his measurements were made.*

I have investigated some of the gauges with a view to discover the basis on which they were originally formed; the irregularities in gradation are very great, but the diameters when plotted as ordinates gave a series approximating to a logarithmic curve such as would be formed by the constant addition of 11 or 12 per cent. to the diameter of each preceding size.

Upon the whole I am therefore inclined to think it probable that the original gauge may have been formed by taking as its basis No. 16 bell-wire, having a diameter of $\frac{1}{16}$th of an inch, and that each succeeding size up to No. 1 was formed by successive additions of 25 per cent. to the weight. This would be equivalent to successive increments of 11·8034 per cent. to the diameters, and would give a logarithmic curve which could be extended indefinitely in either direction.

In the annexed Table I have given columns showing the diameters and sectional areas of wires of the various sizes formed on this principle; that is to say, by assuming ·65 as the diameter of No. 16 wire, and forming the other sizes from it by constant increments of 25 per cent. in weight (or, what is the same thing, by constant decrements of 20 per cent. in weight), and the curve thus formed is delineated in the annexed diagram in juxtaposition with the curve representing the Birmingham Wire-gauge as given by Mr. Holtzapffel. They will be found to approximate so closely to each other and to the Birmingham Wire-gauges at present in use, that I feel confident the proposed gauge might be substituted for the Birmingham Wire-gauge without occasioning any practical inconvenience, and that, if it or any other similarly constructed gauge were issued under the name and sanction of the British Association, it would be universally welcomed by manufacturers, and would speedily obtain general adoption.

* This is identical with the gauge manufactured by Mr. Stubs, of Warrington.

TABLE OF THE SIZES OF THE "BIRMINGHAM WIRE GAUGE," AS GIVEN BY

23	...	...	...	...	...	25	25	28·1	25	27·5	...	24	...	29·7	23
24	...	...	25	...	...	22	22	25	22	25	...	22	25	26·6	24
25	...	...	23	...	...	20	20	22·9	20	22·5	...	20	23	23·8	25
26	...	...	19	...	...	18	18	20·8	18	18·5	...	18	19	21·3	26
27	...	...	18	...	...	16	16	18·7	16	17	...	16	18	19	27
28	...	...	16	...	...	14	14	16·6	14	14·4	...	14	16	17	28
29	...	...	15	...	...	13	13	14·6	12	13	...	13	15	15·2	29
30	...	...	14	...	...	12	12	12·5	11	12·2	...	12	14	13·6	30
31	...	...	12	...	...	10	10	...	10	11·5	...	10	12	12·2	31
32	...	...	10	...	...	9	9	...	9	10·5	...	9	10	10·9	32
33	...	...	9·6	...	...	8	8	...	8	8·5	...	8	...	9·7	33
34	...	...	8·7	...	...	7	7	...	7	7·5	...	7	9·6	8·7	34
35	...	...	7·9	...	...	5	5	...	6	5·5	...	5	8·7	7·8	35
36	...	...	6·7	...	...	4	4	...	5	4	...	4	7·9	6·9	36
37	...	...	5·8	...	...	...	...	...	...	...	...	...	...	6·2	37
38	...	...	4·2	...	...	...	...	...	...	...	...	...	6·7	5·6	38
39	...	...	3·9	...	...	...	...	...	...	...	...	...	...	5	39
40	...	...	3·3	...	...	...	...	...	...	...	...	...	...	4·5	40

* This is the gauge employed by Mr. Stubs of Warrington.

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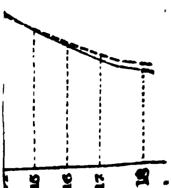


PROPOSED BRITISH GAUGE.

Areas increasing 25 *per cent. Diameters increasing* 11·8034 *per cent.*

Nos.	Areas in Square Inches.	Diameters in "Mils."	Nos.	Areas in Square Inches.	Diameters in "Mils."
00000	·2878182	605·3	19	·0016990	46·5
0000	·2302546	541·4	20	·0013591	41·6
000	·1842037	484·3	21	·0010873	37·2
00	·1473630	433·1	22	·0008699	33·3
0	·1178904	387·4	23	·0006959	29·7
1	·0943123	346·5	24	·0005567	26·6
2	·0754498	309·9	25	·0004454	23·8
3	·0603598	277·2	26	·0003563	21·3
4	·0482879	247·9	27	·0002851	19·0
5	·0386303	221·7	28	·0002280	17·0
6	·0309042	198·3	29	·0001824	15·2
7	·0247234	177·4	30	·0001459	13·6
8	·0197787	158·7	31	·0001167	12·2
9	·0158229	141·9	32	·0000934	10·9
10	·0126583	126·9	33	·0000747	9·7
11	·0101267	113·5	34	·0000597	8·7
12	·0081013	101·5	35	·0000478	7·8
13	·0064811	90·8	36	·0000382	6·9
14	·0051848	81·3	37	·0000306	6·2
15	·0041479	72·6	38	·0000245	5·6
16	·0033183	65·0	39	·0000196	5·0
17	·0026547	58·1	40	·0000157	4·5
18	·0021237	52·0			

ON THE BIRMINGHAM WIRE-GAUGE.

By Latimer Clark, Mem. Inst. C.E.

A Paper read at the British Association for the Advancement of Science, at Exeter, August, 1869.

In a Paper read before the British Association at its meeting in Dundee, in September, 1867, I had the honour of submitting to the Members of this Section a suggestion to promote the establishment of some universal wire-gauge. The gauge which I then ventured especially to recommend was one approximating very closely to the commonly employed Birmingham Wire-gauge, and based upon a simple mathematical series, each number representing a weight 25 per cent. heavier than the succeeding one.

Such a scale would, as I then showed, be for all practical purposes sufficiently near to the existing so-called Birmingham Wire-gauges as to allow of its being, without any inconvenience, introduced into trade and manufacture in their stead. At the same time it would always be conveniently reproducible should ever a reproduction of the normal standard be considered desirable.

The history of the present Birmingham Wire-gauge is as much enveloped in darkness as is the date at which it was introduced; and this is much to be regretted, for, had the gauge been determined upon any rational basis, the readiest way would have been to have returned to this basis and reproduced it. In the absence of any definite information however to lead us to its origin, it remains only to search in the succession of measures of the gauge itself for internal evidence of the considerations, if any, or at least of the law, contemplated or accidental, upon which it was based.

In comparing the curve formed by the successive measures of the Birmingham Wire-gauge with that formed by a logarithmic series, it appeared highly probable that, although very nearly approximating to it, the former measure was not based upon a logarithmic equation, the difference evidently not being one due to difference of constants only.

In the logarithmic series which I suggested, the relation between

the diameters of succeeding numbers is throughout as 1 to 0·8945; whereas in the Birmingham Wire-gauge series this relation, or, if I may be allowed to term it so, the factor of reduction, becomes gradually less, from 0·92, or thereabouts, in the earlier numbers to 0·82, or thereabouts. in the higher ones.

At the time from which the Birmingham Wire-gauge probably dates, the manufacturers who then introduced and employed it were not, I think, of a class likely to call in the aid of either mathematical or physical science to supply them with the groundwork of a gauge. Much more probable, indeed, is it that they took a series of already drawn wires and constructed their gauge directly from these.

Before the introduction of steam-power the largest size of drawn iron wire was that now known as No. 1 B.W.G., having a diameter of about $\frac{3}{10}$ths of an inch. From this the next smaller size was drawn at one operation, and from this in turn a still smaller size, until the last or smallest wire had a diameter of only a few thousandths of an inch.

The most natural way therefore by which the wire manufacturers could provide themselves with a gauge would be to call the largest wire they could draw, No. 1; the next smallest which with their appliances could at one operation be drawn from this, No. 2; the next smallest drawable wire, No. 3; and so on. In doing so, the relations of the succeeding sizes would be determined between two considerations. On the one hand, the manufacturer would naturally endeavour to draw down as small as possible at one operation, in order to save labour; on the other hand, he would be limited in this by the strength of his men or the machinery in use before the introduction of steam-power, and by the cohesive strength of the material itself. Practice would soon show the most profitable mean between these opposing items, and thus the course of manufacture itself would in time supply the workman with a succession of sizes, which he would only need to transfer directly to a measuring plate or callipers in order to have a gauge adapted to every want of his own individual trade.

This I now believe to have been the origin of the Birmingham Wire-gauge.

If this is the case, we should, it is presumable, in investigating the Birmingham Wire-gauge series, find some constant relation between the breaking strength of each wire and the resistance opposed by the draw-plate in drawing it down from its original diameter. Such a relation indeed exists.

If we call the diameter of any given number in the gauge (D), and the next succeeding one (d), the ring section opposed by the draw-plate will be—

$$(D^2 - d^2)\frac{\pi}{4},$$

whilst the resistance (R) opposed in drawing will be—

$$1) \quad R = r\,(D^2 - d^2)\frac{\pi}{4},$$

(r) being the resistance to drawing against the unit of surface.

Further, if (s) is the absolute cohesive strength of an unit section of the material in a hard drawn condition, the absolute strength (F) of the wire as it leaves the draw-plate will be—

$$2) \quad F = s\,d^2\frac{\pi}{4}.$$

Assuming the relation between the absolute strength of the drawn wire and the resistance of the opposing ring to be a constant one, balanced between the items of labour and rupture, say $\frac{R}{F} = m$, then—

$$r\,(D^2 - d^2)\frac{\pi}{4} = m\,s\,d^2\frac{\pi}{4}$$

$$d = D\sqrt{\frac{r}{m\,s + r}},$$

and the factor of reduction, or the relation between the diameter of any wire and that from which it was immediately drawn, would be—

$$3) \quad \frac{d}{D} = \sqrt{\frac{r}{m\,s + r}}.$$

The value of (s) has been as exactly determined for almost all substances as their varying qualities and structural irregularities

allow, and is usually expressed as the weight which would rupture a bar 1 sq. inch section. This value is not constant for all sections, being found to increase in some function as the section decreases. Thus a thousand wires, each $\frac{1}{1000}$th of an inch section, would bear a greater weight than a bar of the same metal 1 sq. inch section. The reason of this is probably, that, when drawn out in the form of wire, irregularities of structure which existed in the bar are for the greater part removed, having caused perhaps the rupture of the wires repeatedly in drawing, and the resulting wires, having been thus "filtered" through the draw-plate, are therefore of a superior quality to the bar from which they were drawn.

Whatever the cause may be, however, the fact remains that the coefficient of cohesive strength of a small wire is greater than that of a large one. Were it not for this, we might assume that the value $\frac{d}{D}$ should be a constant, and the Birmingham Wire-gauge series would then form a logarithmic curve.

As it is, the value of (s) varying with (d), a difference must necessarily be found between the curve representing the Birmingham Wire-gauge and one based upon a logarithmic series.

With regard to the value of the natural constant (r), the only determinations, I believe, are those of Egen, quoted by Mr. H. Thomée in his very valuable paper on Gauges read before the Society of German Engineers in 1866.

These determinations are given in the following columns, in which I have for clearness' sake converted the resistance from German into English pounds.

1.	Iron Wire of	248	mils. drawn	down to	220 mils.	required =	2063	lbs.
2.	„	114	„	„	101	„	400	„
3.	„	101	„	„	91	„	253	„
4.	„	91	„	„	82	„	156	„
5.	„	82	„	„	72	„	164	„
6.	„	72	„	„	53	„	164	„
7.	„	53	„	„	48	„	65	„

This series of experiments allows us to arrive at an approximate value of the natural constant (r), or the coefficient of resistance to drawing per square inch of section of iron, as appears in column 6 of the following Table:—

No.	D.	d.	Area of Ring of Resistance.	Force required.	$r = \frac{\text{Force}}{\text{Area}}$.
1	2	3	4	5	6
	Mils.	Mils.	Square Mils.	lbs.	lbs. per sq. inch.
1	248	220	10,290	2063	200,400
2	114	101	2,195	400	182,325
3	101	91	1,508	253	167,450
4	91	82	1,198	150	127,162
5	82	72	1,210	164	135,800
6	72	63	954	164	172,125
7	53	48	397	65	138,875
				Mean ...	160,591

Assuming the coefficient of strength or the cohesion (s) of a bar of iron 1 sq. in. section to be 80,000 lbs. in round numbers, and that this were constant for all sizes of wire, the absolute strengths of the foregoing wires would be as follows:—

No.	d.	Absolute Strength.	No.	d.	Absolute Strength.
		lbs.			lbs.
1	220	3041	5	72	326
2	101	641	6	63	249
3	91	20	7	48	145
4	82	422			

and the relation of the absolute strength to their resistance to drawing $\left(\frac{R}{F}\right)$:—

$$\left.\begin{array}{ll} 1. & \frac{2063}{3041} = 0{\cdot}678 \\ 2. & \frac{400}{641} = 0{\cdot}624 \\ 3. & \frac{253}{520} = 0{\cdot}487 \\ 4. & \frac{156}{422} = 0{\cdot}370 \\ 5. & \frac{164}{326} = 0{\cdot}503 \\ 6. & \frac{164}{249} = 0{\cdot}659 \\ 7. & \frac{65}{145} = 0{\cdot}448 \end{array}\right\} \text{Mean } \frac{R}{F} = 0{\cdot}538.$$

Assuming this mean 0·538 to be the probable value of the con-

stant (m) for iron and inserting it in equation 3, we get for the factor of reduction between two sizes

$$\frac{d}{D} = \sqrt{\frac{160591}{0{\cdot}538 \times 80000 + 160591}} = 0{\cdot}8881,$$

which is about a mean value of the relations throughout the Birmingham Wire-gauge series.

From this agreement it might, at first sight, seem obvious that, in the above formula, if, instead of the mean coefficient of cohesive strength (80,000), we inserted its variable value answering to the various diameters of the wires of the gauge, we should obtain a series of factors of reduction which would give us a perfect reproduction or rectification of the Birmingham Wire-gauge. This would indeed be the case if we could assume a function of variation with any pretence whatever to its being a constant one. It must, however, be recollected that this variation in the apparent cohesive strength is due, not to any constant physical property or law of the material experimented on, but solely to accidental faults in its structure, in fact to the badness of its quality, an item which varies with every different sample. The true coefficient of cohesive strength of any material is, of course, the highest value obtainable for it; being the value nearest to that obtained with a very thin wire. The coefficients found with larger bars refer not to the material but its faults, and, were it not for these faults, wires of all sizes of the same material would have the same coefficient of cohesive strength.

Following these considerations, the conclusion which we must, I think, arrive at is, that the Birmingham Wire-gauge has been formed from a series of wires drawn from one another, and into which the effects of impurities of the material have entered. Had this not been the case, that is to say, had all the wires from which the Birmingham Wire-gauge series were originally drawn been perfectly pure and homogeneous, there can be no doubt that the succession of numbers would have had a common factor of reduction, and formed therefore a logarithmic curve.

In reconstructing this gauge, the question forcibly presents itself: are we to allow the effects of accidental impurities in the material to form part of the basis of a widely employed measure, or shall

we not rather base it upon the assumption of a pure and homogeneous material?

An objection might be raised to this, that the origin of the gauge would be limited to the physical properties of an individual metal, viz. iron. This is true, probably, in so far as the origin of the Birmingham Wire-gauge goes, but it would appear that for most metals a constant relation exists between their coefficients of resistance to drawing and of absolute strength; and, this being the case, it becomes probable that, had the workers in any other metal determined a gauge, they would with the same method have arrived at the same result.

Karmarsch has given a Table of the relative "drawability" of wires of various metals, taking that of hard-drawn steel as 100. Of course his Table does not profess to give exact quantitative values, but it is sufficient to enable us to see that, for all the metals contained in it, a relation undoubtedly exists between the coefficients of drawing resistance and of absolute strength. In the following Table Karmarsch's relative values are converted into pounds, by assigning to iron the value usually given it in the Tables, and are compared with the common coefficients of absolute strength:—

Metal.	Relative Drawability given by Karmarsch.	Calculated Resistance to Drawing, per sq. inch = r.	Absolute Strength = s.	$\frac{s}{r}$.
1	2	3	4	5
		lbs.		
Hard-drawn steel	100	285,000	125,000	0·439
„ iron	88	250,000	115,000	0·460
„ brass	77	220,000	84,000	0·382
Annealed steel	65	185,000	75,000	0·405
Hard-drawn copper ...	58	165,000	60,000	0·363
Annealed brass	46	130,000	57,000	0·438
„ iron	42	120,000	56,000	0·466
„ platinum ...	38	108,000	47,000	0·435
„ copper	38	108,000	49,000	0·453
Silver	34	97,000	45,000	0·464
Gold	27	77,000	33,000	0·428
			Mean ...	0·430

From values so roughly determined as those given in the second column, the resulting relations found in the fifth column agree sufficiently well to render it a matter of great probability that even a nearer relation exists than that shown here, and therefore that the Birmingham Wire-gauge, rectified to a logarithmic series, is not confined to the physical properties of iron, but is equally applicable to any of the other metals mentioned.

If the coefficients of cohesive strength and of resistance to drawing were known and constant magnitudes, nothing would obviously be easier than to construct, on the probable basis of the Birmingham gauge, a rational and applicable measure. These coefficients are, however, so variable with slight differences in the qualities of materials, that the nearest approach we can make to perfection in a scale based upon their relation to each other is by assuming mean values.

In the British gauge which I have had the honour of suggesting, these coefficients have average values, notwithstanding that I started from a factor of reduction, which I assumed only for the reason of its simplicity, as it allows at any part of the scale the weight of the preceding number to be arrived at by the addition of 25 per cent., and of the succeeding by the subtraction of 20 per cent. of the one from which we start.

The factor of reduction for diameter being therefore 0·8945, and the coefficient of cohesive strength being 80,000,

$$0{\cdot}8945 = \sqrt{\frac{r}{0{\cdot}538 \times 80000 + r}};$$

whence

$$r = 172{,}300,$$

which is rather higher than the mean value deduced from Egen's experiments.

The adoption of some uniform wire-gauge has become a pressing necessity of manufacture, and I submit that the Birmingham Wire-gauge—re-established on a rational basis, and rectified from the inequalities which have crept into it, partly through the want of some recognised standard, and partly by reason of the impurities of the materials from the properties of which it was probably

originally determined—is the best adapted to all the wants of the wire-trade.

In conclusion, I must call the attention of the British Association to the labours of M. Karmarsch, M. Thomée, and M. Peters towards the establishment of a uniform wire-gauge in Germany, whose opinions entirely agree with my own as to the superiority of the English Birmingham Wire-gauge in point of practical utility over all the others now in use, and to whose writings I am indebted for much interesting and instructive information on this subject.

REPORT ON A STANDARD WIRE-GAUGE.

A Paper read before the American Institute of Mining Engineers, at the Amenia Meeting, October, 1877.

[Extracted from the Journal of the Franklin Institute of Feb. 1878.]

The Committee on a Standard Gauge have been constantly engaged since their appointment in the duties assigned to them. They have corresponded with different persons interested in the manufacture and use of such gauges in this country, and have received from several of them important information.

They have also entered into correspondence with the Governments of England, France, Germany, and Russia, through their Consuls, and with Austria directly. The Consuls of Germany and France have taken the greatest interest in the matter, and have communicated to your Committee a large amount of valuable information relating to the gauges used in their countries. Professor Tunner, of Leoben, Austria, one of our honorary Members, has communicated information relative to the use of gauges in Austria. The replies to the communications addressed by the English and Russian Consuls to their respective Governments have not as yet been received.

Your Committee commenced its labours having in view to find a gauge which should be simple in its construction, not readily worn, capable of easy adjustment, and not too expensive to be used

by the ordinary workman. With this in view, they have examined a large variety of gauges, and believe that all those in general use in the United States have passed under their inspection.

We find, as the result of our examination, that, although there are a great number of patterns, most of the gauges in general use differ but slightly in principle. The different systems may be divided into two general classes. These are—*first*, fixed; and *second*, moveable gauges.

Of the fixed gauges there are three general types. These are first, those made with slots, open at one end, the sides of which are intended to be parallel, as the ordinary wire-gauge; second, those made with round holes made in a plate, with or without a plug corresponding to each hole to check the size, such as the Whitworth Gauge and the Stubbs Wire-gauge, better known in this country as the "twist drill" gauge.

In both these kind of gauges the slots and holes are designated by numbers.

The third kind of fixed gauge consists of a V, either cut into a sheet of steel, or formed by placing two bars of steel together at one end, and leaving them open at the other a fixed distance.

Of the moveable gauges there are two types; sliding calipers with verniers, with or without a micrometer screw for adjustment; and the micrometer screw-gauge.

Your Committee find that the gauges which are characterised by round holes or slots, designated by numbers, are only approximately correct. They not only differ according as they are made by different manufacturers, but in a package of a dozen made by the same manufacturer there often were very perceptible and annoying differences. They find that in the gauges with open slots the sides are rarely parallel, and that there are even greater variations in them than in the gauges made with closed round holes without plugs. They find that the numbers affixed to the slots and holes vary so much on account of the differences in the width of the slots and in the diameter of the holes as to be a constant source of inaccuracy, uncertainty, and annoyance. This variation has, in certain cases, been found to amount to as much as 50 per cent. of the weight of different wires of the same number which have been

examined. It is therefore impossible to make even an approximate comparison of sizes, unless, besides the number, not only the kind of gauge, but also the name of the maker, is specified, and that, even then, this approximation cannot be relied upon when the gauges have been worn from constant use or bad tempering.

The best example of the round holes with plugs is the Whitworth gauge, which is made of a thick plate of tempered steel. Each hole of the gauge is provided with a hardened steel plug, which fits it exactly. In all the recent gauges of this kind the system of numbers is abandoned. The plug is made of a given diameter, which is stamped in figures on each one. These diameters, generally, vary by thirty seconds, sixteenths, eighths, quarters, and so on, each size having a hole and plug of its own, so that a complete set will consist of as many holes and plugs as there are fractional parts. To obviate the difficulty of the indefinite repetition of the plugs, they are sometimes made so that when any two, or even three, plugs are placed together, they will exactly fit the hole corresponding to the sum of their diameters. This arrangement is made to insure accuracy, as the multiplication of a very slight error would prevent even two plugs from fitting the hole corresponding to the sum of their diameters. When well made, this gauge is an instrument of precision; but it is evident that, in order to have such a gauge even moderately accurate, it must be a very expensive instrument, and altogether beyond the reach of an ordinary workman, or even of a manufactory with small capital; and that from the indefinite multiplication of holes and plugs it must necessarily be very cumbersome. When they are used, there must always be two such gauges, one for comparison and one for use, and when the gauge is only very slightly worn it ceases to be an instrument of precision, and is then open to all the objections of the ordinary gauge with fixed holes.

Your Committee, very early in the course of their investigation, formed the opinion that no reliance whatever was to be placed on the numbers of gauges as an indication of size except for the individual gauge to which the number was attached; and that the only accurate and scientific way of expressing the size of an article to be gauged was by some expression of its diameter which should

be more exact than numbers, and which would allow of an accurate comparison of all the dimensions by whatever gauge they were taken.

Your Committee are supported in this opinion by the present practice among some European manufacturers who have recently acted in this matter, who have decided that a given number on a gauge shall correspond to a given diameter expressed in fractions of the legal standard of length of the country; but, as in all fixed gauges made for ordinary commercial use the diameter can only be approximately expressed, neither the number nor the diameter is ordinarily correct, so that there is a double source of inaccuracy, as the number does not express the exact diameter nor the diameter the number.

Owing to the great liability to error and the impossibility of correcting it even in the most elaborate forms of this kind of gauge, your Committee, early in the course of its investigation, after having themselves examined a large number, and having had communicated to them the results of examinations made by others, dismissed this class as being unsuitable, either from their defective construction, the impossibility of adjusting them when out of order, or their great cost, from their consideration as a standard gauge.

Your Committee next turned its attention to the V gauge, which is made by placing together two pieces of hardened steel, so that they touch at one end, but are open a given distance at the other, the numbers or diameters corresponding to the opening being graved upon one or both of their sides. The accuracy with which measurements can be made with this gauge when it is new and the jaws properly tempered, adjusted, and fastened, is surprising. Exceedingly minute differences, even in the diameters of the same wires, can be detected and measured with great nicety, but by constant use the gauge wears unevenly. It must then be taken apart, reground, and readjusted, which will generally cost more than the gauge is worth.

Your Committee, while having the highest opinion of it for ordinary purposes, after some months of study, abandoned the idea of recommending it as a standard gauge.

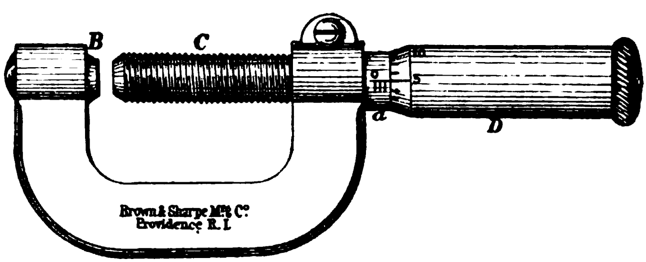
Their attention was then turned to the other two kind of gauges,

namely, the sliding-gauge with a vernier, with or without a micrometer adjustment, and the gauge known as the micrometer gauge. The advantage of these gauges is great accuracy. The sliding-gauge with a vernier necessarily wears, but the error of wear can be ascertained and allowance made for it, so that accurate measurements can always be made with it when it is worn.

In the micrometer gauge the wearing surfaces are so arranged that they can be adjusted with ease in a few moments. The wear between the male and female parts of the micrometer can be adjusted by a binding-screw. This adjustment can be repeated as often as required, so that the instrument will read with great accuracy until it is worn out.

Your Committee assured themselves by actual trial, that with such a gauge boys can very easily be taught to read the thousandth of an inch or the fortieth of a millimetre, and that it is practicable to read even the eightieth of a millimetre.

The micrometer gauge is, of these last two gauges, the simplest. It consists of micrometer screw, C, with a vernier attachment on D, is susceptible of easy adjustment, is not likely to wear, is not complicated, is less likely to get out of order than the other gauges, is more easily read, and requires less skill to read it than the sliding gauge with a vernier. Your Committee are therefore of the opinion



that this gauge, which is shown in the above cut, is the gauge which should be adopted as the standard gauge.*

* Description of the Gauge.—The piece in the form of the letter U has a projecting hub, *a*, on one end. Through the two ends are tapped holes, in one of which is the adjusting screw, B, and in the other the gauge screw, C. Attached to the screw, C, is a thimble, D, which fits over the exterior of the hub, *a*. The end of this thimble is beveled, and the beveled edge graduated into twenty-five parts, and figured 0, 5, 10, 15, 20. A line of graduations, 40 to the inch, is also made upon

They are of the opinion that all gauges should be graduated so as to read fractions of an inch or of a millimetre, and that the sizes should be expressed as the only means of insuring correct measurements, and not by numbers, which constantly lead to error. That this, while it ensures great accuracy, presents no difficulty in practice, is shown by a number of experiments made during a period of several months, to ascertain the practical difficulty in the way of the adoption of this method, by a Member of your Committee. The sizes of some of the steel bars, the orders for which were expressed in thousandths of an inch, are given below.

Sizes expressed in decimals of an inch, taken at random from the order book of a manufactory which has adopted this method :—

15·5 × ·014	3·00 × ·0145	2·25 × ·059
15· × ·02	3· × ·018	2·25 × ·046
15· × ·014	3· × ·02	2·25 × ·040
5·25 × ·061	3· × ·0125	2·25 × ·038
4·50 × ·062	2·75 × ·030	2·25 × ·055
4· × ·024	2·75 × ·051	2·25 × ·020
4· × ·022	2·75 × ·035	2· × ·018
4· × ·071	2·50 × ·059	1·50 × ·032
3·475 × ·062	2·50 × ·022	·75 × ·095
3·25 × ·01	2·25 × ·031	·25 × ·062

The trial of this system by some of the manufacturers has resulted in banishing all the old forms of gauges from their workshops.

The conclusions which have been arrived at, for the most part independently, by the different Members of your Committee, and in which they unanimously agree, are—

the outside of the hub, *a*, the line of these divisions running parallel with the centre of the screw, C, while the graduations on the thimble are circular. The pitch of the screw, C, being 40 to the inch, one revolution of the thimble opens the gauge 1-40 or 25-1,000 of an inch. The divisions on the thimble are then read off for any additional part of a revolution of the thimble, and the number of such divisions are added to the turn or turns already made by the thimble, allowing 25-1,000 for such graduation on the hub, *a*. For example, suppose the thimble to have made four revolutions and one-fifth. It will then be noticed that the beveled edge has passed four of the graduations of the hub, *a*, and opposite the line of graduation will be found on the thimble the line marked 5. Add this number to the amount of the four graduations, which is 100-1,000, and it equals 105-1,000, which is the measurement shown by the gauge.

1. The abandonment of the system of fixed gauges for commercial use.

2. The abandonment of the system of representing the diameters and sizes by numbers.

3. The adoption of the system of expressing sizes in thousandths of an inch or fractions of a millimetre.

4. The adoption of the micrometer gauge as the method of measuring sizes.

Your Committee beg to acknowledge their indebtedness to J. B. Knight, Secretary of the Franklin Institute in Philadelphia, for the Reports of various Committees on gauges of the Franklin Institute; to C. Hewitt, Esq., President of the Trenton Iron Company, for a large number of measurements of wire made with different gauges; to P. Ritter von Tunner, of Austria, for the description of the kind of gauges used in Austria; to the German Consul, for his interest in procuring from Germany a report of their gauge system; to the French Consul, for his interest in the work of the Committee; and to the Minister of Agriculture, Commerce, and Public Works, for a complete description of the gauge system as used in France.

Your Committee is, however, particularly indebted to Darling, Brown, and Sharp, of Providence, who have loaned to them without charge all the gauges which they manufacture, for comparison, and have contributed besides a very large amount of information on various matters connected with this subject.

All of which is respectfully submitted.

T. Egleston, *Chairman.*
Wm. Metcalf.
Jos. D. Weeks.

LESUERRE'S HELIOGRAPH.*

Report made to the Academy of Sciences by M. le Maréchal Vaillant.†

Translated from the French and communicated by R. S. Brough, Member (with a Drawing of the Instrument as constructed by M. Moltein.)

From the complete success of the experiments that have been made it may be asserted that a method of telegraphing has been discovered for Algeria at once inexpensive, rapid, and which, above all, is able to directly traverse the greatest distances. Southern Algeria, which will not lend itself to the construction of ordinary lines of telegraph, is, on the other hand, eminently suited for the Heliograph. The stations might be placed at twenty leagues from one another in the oases which rise out of these distant plains. The quickness with which it can be set up, as well as its lightness, makes it an excellent portable telegraph.

We extract the following description of the instrument, and statement of the results obtained, from the author's Memoirs and from the Report of the Director of the Observatory.

The method is based on the reflection of the sun's rays by a plane mirror. Three points have to be considered, namely:—

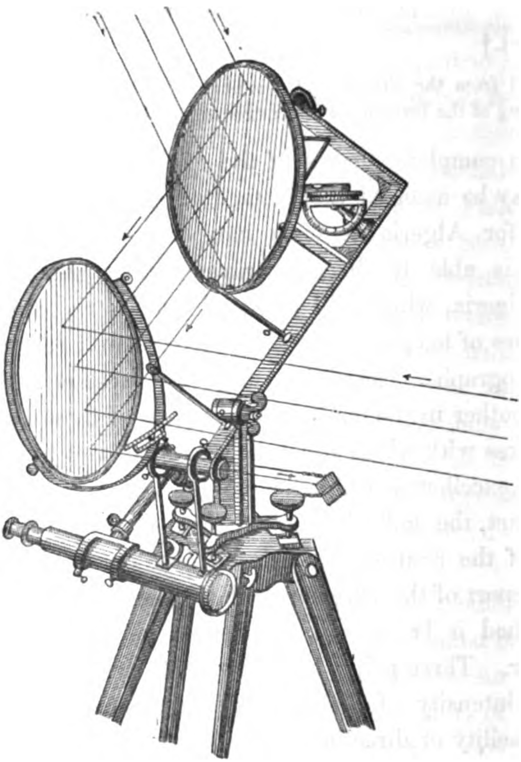
1st. The intensity of the light reflected at a great distance; 2nd. The facility of directing the reflected pencil of light to any given point; and 3rd. The nature of the signals to be employed.

The reflected pencil of light forming a cone of 0° 32′ aperture (the apparent diameter of the sun) affords a sufficiently large field to prevent inconvenience from any slight error in the adjustment of the instrument. To ascertain the direction of the reflected pencil, a small astronomical telescope is placed in its path, so that its eye-piece projects on a screen fixed behind it, the reflected image of the sun and the shadow of the cross-wires of the collimator.

* Jules Lesuerre was admitted to the "Ecole Polytechnique" in 1848, and in 1851 was nominated Pupil-Inspector of Telegraphs. He died, aged 36, at Pau, in February 1864, after a long illness induced by exposure incurred in the execution of his duty in the field in Algeria.

† Comptes Rendus, Séance 16th June, 1856.

The relative position of the projected solar disc and of the point of intersection of the wires will correspond with that of the pencil of light with respect to the optical axis of the telescope.



When the point of collimation falls in the centre of the disc, then the direction of the optical axis of the telescope is parallel to the direction of the axis of the reflected pencil of light. If, on the contrary, the point of collimation is near the edge of the disc, then one of the limiting edges of the conical pencil is in mean parallelism with the optical axis of the telescope (and the mirror requires adjustment).

If now we know the direction of the collimating telescope we can infer the direction of the pencil of light. To this end the small "collimating" telescope is mounted, like the "finders" employed in connection with astronomical instruments, on a longer "observing" telescope; the two have their optical axes parallel, but

face in opposite directions.* When we desire to direct the optical axis of the collimating telescope towards a particular point we have only to look through the observing telescope and adjust it till it covers the desired point. By then looking at the screen we can see whether the distant point falls with the field of the reflected pencil of light or not, and adjust the mirror accordingly.

The question of direction is so simplified by this procedure, that, when the collimating telescope is properly directed, the mirror can be adjusted by hand, or, for the sake of convenience, be mounted on a stand and adjusted by two tangent screws.

For field triangulation it would suffice to add to the instruments of each brigade a glass a few square decimetres in area to get "sights" visible at very great distances.†

The signals consist of series of short or long flashes, produced by withdrawing for longer or shorter intervals a screen which, in the normal state of the instrument (at rest), intercepts the reflected pencil of light. The Morse alphabet can be adopted in its entirety, the short and long flashes taking the place of dots and dashes respectively. In fact the sun could himself write the signals on sensitive paper moving uniformly in front of a lense.

Such as it has been described, heliography labours under a defect, namely, that near sunrise and sunset the quarter of the horizon opposite to the sun can only receive extremely feeble (reflected) light; for the surface of the mirror, which then forms an exceedingly acute angle with the reflected rays, presents an almost insensible apparent area.

This defect is remedied by the addition of a second mirror. This apparent complication of the instrument in reality simplifies the manipulation of the instrument, and offers important advantages. The apparatus then forms a heliostat with two mirrors, one of which is moveable, and reflects the rays of the sun toward the pole; the other is fixed, and receives the rays from the first and transmits them to the distant station.

* Of course the little collimating telescope has its object-glass facing the reflecting mirror, while the object-glass of the large telescope is turned towards the distant signalling station.—R. S. B.

† The "Heliostats" of the Great Trigonometrical Survey of India.—R. S. B.

In front of the second mirror is fixed the "collimating" telescope; and, since it shows the final direction of the emergent rays, all precision in the direction of the axis of the first mirror is dispensed with. The only inconvenience arising from want of accuracy in the adjustment of the first mirror would be the necessity of re-adjusting the second mirror from time to time, in order to keep the shadow of the cross wires at the centre of the reflected solar disc.

The direction of the first reflection may be either towards the north pole or towards the south pole; that one is preferred which makes an acute angle with the direction of the second reflection.

In the case of a permanent signalling station, the adjustment of the axis of the first mirror can be very approximately obtained by star observations; the other part of the installation presents little difficulty. The interceptor is formed of a metallic *persienne* (Venetian blind) with very thin blades pivoted at the ends, so as to be able to be all turned together by means of a connecting rod.

This *persienne* is attached to the axis of the first mirror, and in its normal state (at rest) intercepts the sun's rays, and prevents them reaching the moveable mirror. When we desire to make a signal, the connecting rod is depressed by means of the finger. The blades are turned edgewise to the sun, whose rays they thus allow to reach the mirror, and they fall back to the normal point as soon as the pressure ceases. Thus the mirrors are only exposed to the sun's rays during the actual formation of the signals.

A trial made on the 30th March, 1856, at 3 o'clock p.m., between the tower of St. Sulpice and the tower of Montlhéry, in the presence of MM. Le Verrier, Director of the Paris Observatory, Liais, Astronomer in the same Observatory, and Struve, Astronomer to the Russian Observatory at Polkova, gave the following results:—

Flashes almost blinding to the naked eye, notwithstanding a fog; correspondence rapid and without any hitch; flashes sensible to the naked eye, and very brilliant through a telescope, when the sun was obscured by white clouds.

The glasses employed were ordinary commercial mirrors, 0·12 square metres ($=1\frac{1}{3}$ sq. ft.) in area, which had been left exposed to all the vicissitudes of the weather during four months, and

mounted on rough apparatus made up by a carpenter and a locksmith.

The portable heliograph weighs 8 kilogrammes (= 18lbs. avoirdupois), is mounted on a wooden tripod, and is adiusted by means of a compass-needle and a spirit-level.

The installation occupies scarcely a minute. To simplify the arrangements the interceptor may be suppressed: the stationary mirror is normally deflected from its proper position, into which it is brought at pleasure by pressing a key with the finger, which brings it up against a fixed stop—according to the duration of the pressure a "dot" or "dash" is produced.

Two persons placed within view of one another at a distance of ten leagues, can, by means of this apparatus, find one another and enter into correspondence.

The arrangement of the instruments is in effect such that the one axis of the second mirror can be set vertically while the axis of the first mirror is set horizontally. The solar rays, reflected horizontally by the first mirror, strike the second; and, by turning the latter about its vertical axis, a zone half a degree wide will be described by the base of the reflected pencil of light. Thus the whole horizon can be swept to attract the attention of any one with whom one seeks to open correspondence.

The latter, seeing the point whence the rays emanate, directs his instrument to this point, and sends a continued flash, towards which again the second apparatus can then be adjusted.

In this adjustment one is also of course guided by the "collimating" telescope to correct any errors due to a hurried setting-up of the apparatus.

The portable heliograph tried at the Observatory in the presence of the Minister of War, the Director-General of Telegraphs, and the Director of the Observatory, gave the most satisfactory results.*

* Lesuerre's Heliographs can be procured from the manufacturer, M. Moltein, Rue du Château d'Eau 44, Paris.—R. S. B.

ELECTRO-HARMONIC TELEGRAPHY.*

By F. L. Pope.

A Paper read before the annual meeting of the American Electrical Society, at Chicago, December 12, 1877.

[From the Journal of the American Electrical Society, vol. ii., No. 3.]

Let us, in imagination, transport ourselves backward over a period of three centuries. It is a summer evening in the ancient Italian city of Pisa—a city whose curious leaning tower and imposing cathedral have been reckoned for centuries among the architectural wonders of the world. Beneath the lofty ceiling of the great cathedral, a magnificent central chandelier, suspended by a slender silver chain, swings slowly to and fro in the gentle southern breeze that steals through the open arches. From his station in the chancel, idly at first, then eagerly and intently, a grave-faced choir-boy follows with his eyes the cluster of glittering lamps, as ever and anon a sudden current of air sets it swinging in a wide arc, and then, ceasing for a time, allows the motion to die away in gradually-lessening oscillations.

What could there have been in this simple occurrence which so interested the youthful observer in the chancel? It was this: He had noticed, what doubtless many others had noticed before, but without in the least apprehending its significance, the fact that the oscillations of the suspended chandelier, whether great or small, were always, without exception, performed in *equal times*. Our choir-boy, although a mere youth, had nevertheless already become something of a philosopher, and his subsequent reflections upon the remarkable fact which had thus incidentally attracted his attention led him directly to the discovery of one of the most comprehensive and far-reaching of all physical laws—the law of *isochronous vibration* (the word isochronous being derived from the Greek, and meaning "in equal times"). This discovery was but the first of a

* The blocks of illustrations for this paper are kindly supplied by Mr. Pope.

long and brilliant series which have justly rendered the name of Galileo for ever immortal in the annals of science and of history.

In order that we may arrive at a clear understanding of the principles underlying the different varieties of the telephonic, or, in more general terms, the electro-harmonic, system of telegraphy, and that we may be able to trace intelligently its origin and development, it is essential that we should first become somewhat acquainted with the laws and leading phenomena of vibratory or undulatory motion in general. Having done this, we shall find no difficulty in passing to the consideration of the special practical applications of these laws which have recently been made in the domains of electro-telegraphy and electro-acoustics, and which have been attended with such remarkably brilliant and successful results.

Let us consider for a moment some of the peculiar properties of a body freely suspended from a fixed point—in other words, a pendulum. I suppose there are not many here present who do not treasure among the happiest memories of childhood the associations connected with the swing. It was simply a seat suspended by two ropes, perhaps from the horizontal branch of some overshadowing tree. I shall probably be safe in assuming that you all have a tolerably vivid recollection of most of the phenomena presented by this mechanical contrivance when in active operation; a very fortunate circumstance, inasmuch as it will enable me to place clearly before your minds some of the most important of the fundamental laws of vibration.

When our friend the school-boy, having seated one of his youthful favourites in the swing, and by a series of judiciously-timed impulses gradually increased the amplitude of her oscillations from zero to perhaps 120° of arc, proceeds, in compliance with her breathless request, to discontinue his exertions, and in the classic language of the play-ground to "let the old cat die," it is hardly surprising that, not being another Galileo, our young friend has utterly failed to grasp the great physical truth that the vibrations of the little maiden are isochronous. Still less does he probably suspect that, even were he to subject the very schoolma'am herself to the same conditions, the periodicity and the isochronism of her oscillations would not differ from those of her predecessor, not-

withstanding the much greater weight of the oscillating body. Nevertheless, such is the fact. It is one which was experimentally demonstrated many years ago—by myself, although of course it would hardly be becoming for me to claim absolute priority over all others in making the experiment.

Another important property of the pendulum is, that, by shortening it, it oscillates more rapidly. Thus, if we take two pendulums, one of which is three and the other twelve feet in length, the shorter pendulum will be found to make two oscillations to each one of the longer one, and, if we continue the experiment with pendulums of different lengths, we shall arrive at the law that the time required in each case to perform an oscillation is proportional to the square root of the length of the pendulum.

I will also call attention at this point to a third property of the vibrating pendulum, which it will be very important for us to remember, in view of what we shall come to further on; a property which is very well illustrated by the suspended swing, to which I have just referred. It is this: a freely-suspended body, even if it be very heavy, may be set in vibration by the repeated application of a comparatively insignificant force, provided the successive applications of the force be properly timed, but not otherwise. Of course you have all noticed this in the case of the swing, and therefore I need not enlarge upon it further than to say that the same effect is produced, though in a less degree, no matter whether the impulses are given at every vibration, at every alternate vibration, or even less frequently. The essential condition is that the intervals of time between the successive impulses shall be exactly the same as the intervals between the vibrations, or else a multiple or sub-multiple of one of these intervals.

I have made use of the suspended pendulum to illustrate some of the principal laws of vibratory motion, for the reason that its phenomena are familiar to you all, not merely because they are of every-day occurrence, but because they are very easy of comprehension both by the eye and mind. But the laws which govern the vibrating pendulum equally govern all the varied phases in which vibratory motion presents itself throughout the realm of physics.

All solid bodies exhibit the phenomena of vibration in various forms and degrees, according to the form of the body and the manner in which the force producing the vibration is applied. Cords and wires, as familiarly seen in stringed instruments of music, have their elasticity developed by tension so as to become

Fig. 1.

capable of vibration. If the cord a f b (fig. 1) be drawn out in the middle to a c b, upon being released its elasticity causes it to return to its former position. The velocity of this movement is constantly accelerated, and is at its maximum when the cord has reached its line of equilibrium a f b, consequently it passes with constantly decreasing velocity to a d b, where it comes to rest for an instant, and then returns to a f b, and so continues. You will at once perceive the analogy between the vibrations of the central point f of the string between c and d and that of the weight of the pendulum, and like those of the pendulum the vibrations of the stretched string are isochronous. It may be regarded, in fact, as a kind of double pendulum, and is subject to the same laws as the ordinary pendulum. The tension and thickness being equal, the number of vibrations performed by a cord in a given time are inversely as its length. Elastic rods vibrate laterally like cords when fixed by their extremities. In consequence of their rigidity, however, they may be made to vibrate when fixed only at one extremity. Thus, a straight steel rod, n o, may be clamped in a vice, as shown in fig. 2. If we draw the free end n aside and then liberate it, it will vibrate to and fro between the points p and p as shown by the dotted lines. The amplitude of the successive vibrations, however, constantly diminishes, until at length the rod returns to its original state of rest. Such a rod, when vibrating, follows the same law as the pendulum and the stretched cord, each vibration, whether greater or smaller, being performed in the same length of time,

and the number of vibrations in a given time being inversely proportional to the square of the length of the rod.

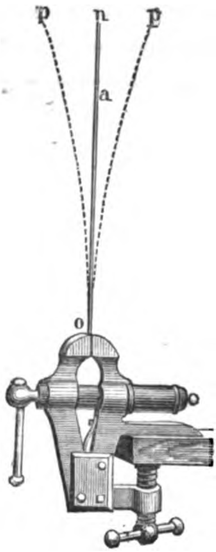


Fig. 2.

The ordinary tuning fork, an almost indispensable instrument in the experimental investigation of the various problems of acoustics, consists virtually of a double vibrating rod of the above character. As actually constructed it is simply a steel bar, bent into the form of an elongated letter U, and supported or clamped at the middle of the bend, leaving the extremities free to vibrate. When such a fork is struck, and thrown into vibration so as to sound its deepest note, its free end oscillates, as seen in fig. 3, where the prongs vibrate between the limits b n and f m, p and q being points of no vibration, termed *nodes.**

Elastic plates are easily thrown into vibration, but the character of their vibrations depends upon the configuration of the plate, the manner in which it is supported or clamped, and the point at which the exciting or moving force is applied. For example, a circular plate, or a plate of any regular geometrical figure capable of being circumscribed about a circle, which is clamped or stopped at the edges, but otherwise free to vibrate, will have no decided tendency to any given rate of vibration, but will respond to any kind of vibrations which may be communicated to it. But, if the plate be elongated, the normal rate of vibration is affected by the length of the plate, without reference to its breadth. The greater the length of the plate in proportion to its breadth, the more it partakes of the character of an elastic rod or a stretched string, according as it is supported at one or both ends, and thereby becomes capable of vibrating at one particular rate and no other. You will see, therefore, that we may have a succession of plates of various forms, passing by degrees from the circular plate clamped at its edges,

Fig. 3.

* Tyndall, *Lectures on Sound* (American edition), p. 138.

which will take any rate of vibration with equal facility, to the string or rod clamped at one or both ends, which will only take one particular rate, rejecting all others. These properties of plates of different forms, in respect to their modes of vibration, are of the utmost importance in harmonic telegraphy, as we shall hereafter see.

It remains to speak of the vibrations of membranes, which are in many respects analogous to those of plates. When loosely stretched over a circular hoop or frame, such a membrane, like the circular plate, has no decided tendency to vibrate at any particular rate. If strained more tightly, however, its tendency to vibrate at some particular rate is increased.

Omitting for the present a more particular consideration of the characteristics of vibrating solids, we will now examine the effects of vibratory motion upon fluids.

If we drop a smooth, round pebble into the bosom of a placid pool, a series of concentric undulations is produced. Wave follows wave, in ever-widening circles, until opposing forces at length cause an equilibrium to be regained. At the initial point a depression is produced by the fall of the pebble. Around this there first rises a circular elevation above the surface of the liquid when in equilibrium, and immediately beyond this is a circular depression, and so on, alternately, successive elevations and depressions. When we look at this progressive series of waves, the entire mass appears to advance progressively in every direction away from the point of excitation; but, if we watch the movements of some light, floating body, we shall see that this body is not carried forward over the surface, but merely rises and falls alternately as the waves pass beneath it. Moreover, we shall be able to observe an exact analogy between the vertical oscillations of this floating body and those of the suspended weight of the pendulum, or the central point of the stretched string, thus proving that the vibratory motion which we have already examined, and the undulatory motion under consideration, are manifestations of the same law under different conditions.

The undulations which we have just described are surface waves. All elastic media are also subject to undulations of a totally different character, which are termed waves of *condensation* and *rarefaction*,

and are produced in air and gases by any disturbance of density. If any elastic fluid be compressed, and then suddenly released from compression, it will expand, and in its expansion exceed its former

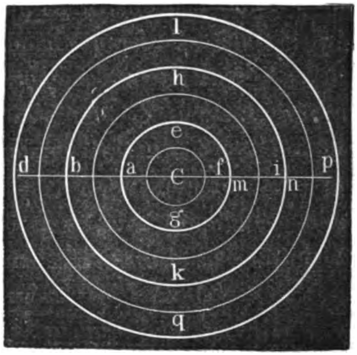


Fig. 4.

volume to a certain extent, after which it will again contract, and thus oscillate alternately on either side of its position of rest. It must be understood that this class of undulations extend equally in every direction from a centre toward every point of the circumference of a sphere. This alternate condensation and expansion of an elastic fluid or medium, extending spherically around the original centre of disturbance, is perfectly analogous to the series of circular waves which we have seen formed around a point of depression on the surface of a liquid, the condensation of the elastic fluid corresponding to the elevation of a surface wave, and the phase of rarefaction corresponding to the phase of depression.

Suppose fig. 4 to represent a section of a sphere of air, or other elastic medium, in which the waves of condensation and rarefaction have extended outward from the center c, then the heavy lines a e f g, b h i k, and d l p q, will represent the phases of greater condensation, the finer intermediate lines will represent the spaces of greatest rarefaction, and the distances m n and n o, between circles of greatest condensation, will be the length of the waves.

These waves of condensation and rarefaction in an elastic medium, like the waves on the surface of a liquid, are subject to the ordinary laws of vibration, and are capable of producing, or of being produced by, the vibrations of a solid body.

The mutual convertibility of vibrations and undulations may be shown by experiment. If a tuning-fork is struck, or excited by a violin bow, and its motion allowed to gradually die away, its prongs oscillate backward and forward in the same manner and after the same law as a pendulum, except that they make many hundred vibrations for each single vibration of the pendulum. A particular tuning-fork, therefore, will always perform a given number of vibrations in a unit of time. This number depends solely upon the construction of the fork, and can, therefore, neither be increased nor diminished, unless the form or properties of the fork are in some way changed.

If we throw such a tuning-fork into vibration, the vibrations of the fork cause undulations in the surrounding air, which are propagated in every direction. How is this brought about? Each of the prongs beats the air in opposite directions at the same time. Let us try to picture to ourselves the physical condition of the air in front of one of these prongs. As the latter strikes outward, the air in front of it will be driven outward, condensed, and, on account of the elasticity of the air, the condensation will at once start to travel outward in every direction, a wave of denser air; but directly the prong recedes, beating the air back in the contrary direction, it will, of course, rarefy the air in front of the prong. But the disturbance we call a rarefaction is propagated in air with the same velocity as a condensation. We must therefore remember that just behind the wave of condensation there is a wave of rarefaction, each travelling with the same velocity, and therefore always maintaining the same position in relation to each other. Thus the fork vibrates a certain number of times in a second, and will consequently generate an equal number of these waves, all constituted alike, and the same length. (See fig. 5.) Suppose a fork to make one hundred vibrations per second: at the end of the first second the wave generated by the vibration at the beginning of the second would have travelled, say, eleven hundred feet (which is known to be approximately the distance traversed in a second by aerial vibration), and the intermediate waves would be uniformly distributed over the intervening distance; that is to say, in eleven hundred feet there would be one hundred waves, each

of them evidently being eleven feet in length. If the fork made eleven hundred vibrations per second, each of these waves would

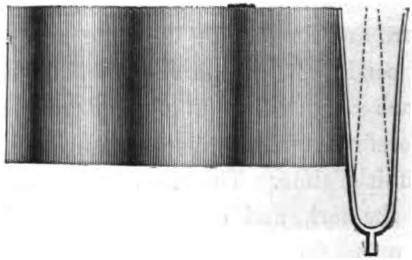


Fig. 5.

be one foot long, for waves of all lengths traverse the air with precisely the same velocity.*

Now, if we place in another part of the same room another fork, so constructed as to make exactly the same number of vibrations per second as the first one, and set the first one in vibration, the other one will soon begin to vibrate in sympathy, and it will even continue to vibrate after the first one has been stopped. Astonishing as it seems, it is nevertheless true that this heavy and rigid mass of steel has been set in motion merely by the successive impact of hundreds of tiny waves of air, each of such small motive power that it could not stir the weakest spring which was not adjusted in unison with the fork. The slightest disagreement in the respective rates of vibrations of the two forks sensibly diminishes, and a difference of one vibration in two or three hundred per second wholly destroys, the effect.

Thus we see that the isochronous vibrations of the first fork give rise to corresponding waves or undulations of condensation and rarefaction in the air, and these in turn reproduce isochronous vibrations in the second fork, and will also produce vibrations to a greater or less extent in every body which is capable of vibrating in unison with the first fork.

Thus far we have confined our attention solely to the nature and effects of simple vibrations. It remains to consider what effect is produced when a number of distinct sets of vibrations are simultaneously propagated through the same medium. Before attempt-

* Dolbear, *The Telephone*, p. 63.

ing to explain this, it is desirable that we should understand the graphical method of delineating vibratory and other motions which

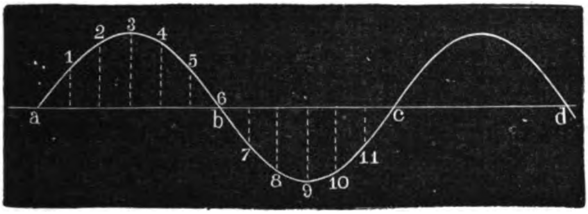


Fig. 6.

mathematicians and physicists are accustomed to employ in order to place the characteristics of these motions before the mind through the medium of the eye, in a manner much more intelligible than is possible even by the most minute verbal description.

Suppose we have a pendulum, swinging from right to left and from left to right, with a uniform motion. In the vicinity of either end of its path it moves slowly, and in the middle much more rapidly. If we should attach a pencil to the end of the pendulum-rod so that it would mark upon a continuous strip of paper of sufficient width, moving uniformly beneath it at right angles to the plane of its oscillation, a wavy line would be produced. This wavy line once drawn would remain as a permanent record of the kind of motion performed by the pendulum during every part of its oscillation. Fig. 6 represents a line such as would be produced by the process we have just described. It is not difficult to comprehend the meaning of the curves which are thus formed. The marking point has passed, relatively to the paper, with a uniform velocity in the direction a d. Suppose it has described the section a c in one second. Divide a c into twelve equal parts, as in the figure, then the point has been one-twelfth of a second in describing the horizontal length of any one of these divisions, and the curve shows us on which side and at what distance from the position of rest the vibrating point will be at the end one-twelfth, two-twelfths, and so on, of a second, or generally at any given short interval of time after it has left the point a. We see, in the figure, that after one-twelfth of a second it had reached the height 1, and that it rose

gradually till the end of three-twelfths of a second; then, however, it began to descend gradually, till at the end of six-twelfths of a second it had reached its mean position b, and then it continued descending on the opposite side till the end of nine-twelfths of a second, and so on. We can also easily determine where the vibratory point was to be found at the end of any fraction of this twelfth of a second. A diagram of this kind, therefore, shows at a glance at what point of its path a vibrating particle is to be found at any given instant, and thus gives a complete image of its motion.*

Although we are not yet able to make all vibrating bodies automatically record their movements on paper in this manner, yet we may ourselves construct curves which truthfully represent their vibration when the law of their motion is known; that is, when we know how far the vibrating point will be from its mean position at any given moment of time. We set off on a horizontal line, such as a b, Fig. 6, lengths corresponding to the interval of time, and let fall perpendiculars, or, in mathematical language, ordinates, to it on either side, making their lengths equal or proportional to the distance of the vibrating point from its mean position, and then, by joining the extremities of these perpendiculars, we obtain a curve such as the vibrating body would actually have drawn if it had been possible to make it do so. Physicists, therefore, having in their minds such curvilinear forms, representing the law of the motion of vibrating bodies, are accustomed to speak as a matter of convenience of the *form of vibration* of such bodies,† a term which I shall hereafter employ when referring to the subject.

We are now ready to return to the consideration of the phenomena of compound vibrations. To illustrate in a general way the characteristics of this kind of motion, we may conveniently refer again to the waves formed upon a calm surface of water. We have seen that, if this surface is agitated by a pebble dropped upon it, the agitation is propagated by concentric waves extending in every direction from the centre to a greater and greater distance. Now, if we drop two pebbles at two points some little distance from

* Helmholtz, *Die Lehre von dem Tonempfindungen* (English Translation, by A. J. Ellis), p. 31.

† *Ibid.* p. 32.

each other, we shall produce two separate centres of agitation. Each will set in motion a separate set of concentric waves, and these two, gradually expanding, will finally meet and overlap each other. When this happens, it is easy to see that not only the water, but any floating body upon its surface as well, will be set in motion by both kinds of agitation at the same time; but this fact will in no wise interfere with the separate propagation of both sets of waves. Each of these will continue to advance further and further over the surface, precisely as if the other had no existence. As they proceed, those parts of both rings which have just coincided appear again, distinct and unchanged in form. These little systems of waves may be accompanied by other and larger systems, caused by the action of the wind; but they will continue to spread out over the surface thus agitated, with the same systematic regularity that they did upon a perfectly calm surface.

The action of the vibrations or undulations of the atmosphere, which produce the sensation of sound, is strictly analogous to that of the waves of water. There is practically no limit to the number of distinct sets of vibrations which may be going on at the same time, without mingling with each other; but, in cases where there are many of these, the resulting motion of each separate particle of air is necessarily complex, almost beyond the power of the mind to conceive. The principle, however, may be understood perfectly well by studying the composition of two or three sets of simple vibrations, and this may be readily done by the aid of the method of graphic projection, which has been before explained.

Thus, in fig. 7, we may suppose the horizontal length of the diagram to represent a unit of time. The curve A will then represent the undulations in the atmosphere caused by the vibrations of a tuning-fork in action. The horizontal distances measured on the straight line will represent the passing time, and the vertical heights the corresponding displacements of the particles of air. Now, suppose a second fork is set in action, which is tuned an octave higher than the first, and, consequently, makes twice as many vibrations in the same time. The undulations produced by the second fork will be represented by the curve B. In such case, the curves above the horizontal line represent the

compression of the air, and those below the line its rarefaction. Now, according to the laws of mechanics, if two different forces act in the same direction, the total force is represented by their

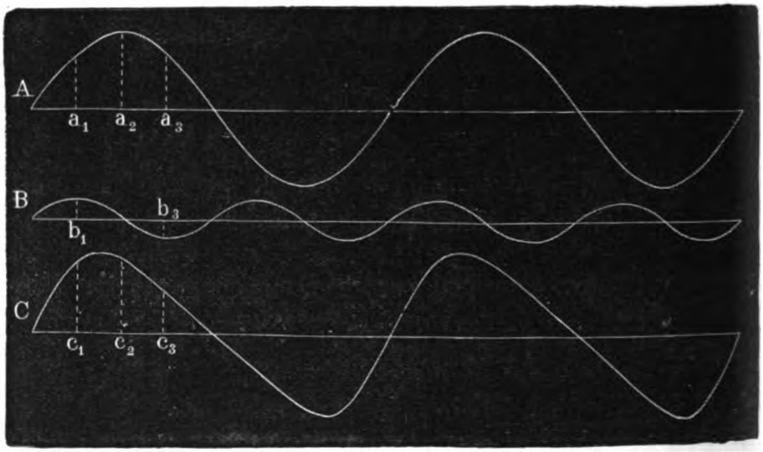


Fig. 7.

sum, while if they act in opposite directions it is represented by their difference. If, therefore, we combine these two simple curves, according to this principle, we shall have a composite curve C, which represents the effect produced by the superposition of one set of waves upon another. The line c_1 is the sum of the lines a_1 and b_1, while c_2 is exactly equal to a_2. On the other hand, the line c_3 represents the difference between the lines a_3 and b_3, one being above the horizontal line and the other below it. Every point in the curve C may be found in the same manner, and, by the same method of construction, the resultant curve corresponding to any number of simple curves combined together may also be found, as you will readily understand.

The simple vibrational *form* is always the same. It is only its wave height or amplitude, and its wave length or periodic time, which is susceptible of change. But the number of vibrational forms which may arise from the composition of simple forms are mathematically infinite. The converse of this proposition is also true, which is, that any form of vibration, no matter how complex, may be expressed as the sum of simple vibrations. This was first

mathematically demonstrated by Fourier, but its experimental proof is due to the labours of the great German physicist Helmholtz, who, after a most elaborate series of investigations, succeeded in separating from each other the several simple sounds which form the constituents of a composite sound. It is not necessary here to enter into a description of the methods employed by Helmholtz in accomplishing this beautiful result,* although we shall have occasion to refer hereafter to some of the analogous means which have been employed in telegraphy for the same purpose, that is to say, the analysis of composite vibratory motions.

The idea of synchronizing the movements of two instruments at widely-separated points, for telegraphic purposes, by making use of the principles of isochronous vibration, was employed in telegraphy at a very early period. Thus, Ronalds † in 1816, and Vail ‡ in 1837, employed isochronous pendulums to control their machinery, while at a later date the printing telegraph of Hughes,§ and the automatic telegraphs of Caselli and others, have embodied most ingenious and beautiful applications of the same principle, with which I presume you are all more or less familiar, and therefore I need not dwell upon them.

In 1861, Mr. Philip Reis, of Germany, made the first apparatus of which we have any account, for reproducing musical sounds at a distance, by means of electro-magnetism. His devices were very ingenious and beautiful, and it is evident from descriptions and papers published at that time,‖ one of which has recently been reproduced in the *Journal of the Telegraph*, that Reis had made a thorough study both of the laws of electro-magnetism and of acoustics, and understood perfectly the conditions of the problem with which he undertook to deal.

* For a full account of the apparatus and methods employed in these experiments, see Helmholtz, chapter iii.

† See Shaffner, *Telegraph Manual*, p. 147.

‡ Vail, *Electro-magnetic Telegraph*, p. 159. Shaffner, *Telegraph Manual*, p. 382.

§ Prescott, *History, Theory, and Practice of Electric Telegraph*, p. 139. Also same author's *Electricity and Electric Telegraph*, p. 609.

‖ Reis, *Dingler's Polytechnic Journal*, vol. clxviii. p. 185. Legat, *Zeitschrift des Deutsch-Osterreichischen Telegraphen Vereins*, vol. ix. p. 125. An excellent translation of this last paper may be found in the *Journal of the Telegraph*, vol. x. p. 353.

Sound is simply a sensation resulting from the action of vibrations upon the nerves of the ear. If the same vibrations are felt by the touch, they produce a certain peculiar fluttering sensation, but this is not sound. Therefore, although all sounds are necessarily the result of vibrations, all vibrations do not necessarily produce sound. The vibratory motions proceeding from sounding bodies are usually conducted to the ear through the medium of the atmosphere. Therefore, to produce any given sound, of whatever character, at a distance, it is evidently only necessary to throw the atmosphere at this point into vibrations precisely similar in every respect to those which would be produced by the action of the original source of sound, whatever it may be.

It is found that all the characteristics of sound which are appreciable by our senses depend upon three things: *First*, the rapidity of the vibrations, which determines what we call the pitch of the sound, whether, for example, it is high or low; *second*, the amplitude of the vibrations, which determines the loudness or power of the sound; and, *third*, the form of vibration as represented by the curve corresponding to the movement of the vibrating body, which determines the quality of the sound.

The apparatus of Reis consisted of a thin stretched membrane, rigidly supported at the edges, and free to vibrate in the middle. The mathematical theory of the vibration of such a membrane, having a uniform tension in all directions, shows that vibrations produced in any part of the membrane will produce nearly as strong vibrations (disregarding individual nodal lines) in all other parts of it. A thin light membrane is not only susceptible of sympathetic vibration when vibrating air is allowed to act upon it, but this vibration is not limited to any particular pitch, and it is therefore capable of responding to sonorous vibrations of every character traversing the atmosphere. A delicate circuit-breaker attached to the membrane was arranged to break the circuit of a telegraph line at each vibration, and thus the armature of an electro-magnet at the receiving station was easily adjusted to respond to these vibrations, and, when mounted upon a proper sounding-board, gave them out to the atmosphere, which conveyed them to the ear of the listener.

Now, if the form of vibration in this sounding-board could have been made to coincide in all respects with that of the membrane at the station from which the vibrations had been transmitted, Reis would have had a perfect sound telegraph or telephone. But this was far from being the case. The pitch and rhythm of the sounds were perfectly preserved; their loudness or intensity also, to a very small extent; but the quality was entirely lost. It is not difficult to understand the reason of this. Every vibration of the membrane caused a pulsation of electricity to traverse the wire and act upon the electro-magnet, but, as each and every vibration of the armature was produced by a current of precisely the same strength, the only difference in the amplitude of these vibrations would be that due to the more complete magnetisation or demagnetisation of the electro-magnet, when the time allowed for the process was increased by the greater play of the circuit-closer, under the influence of stronger vibrations at the transmitting station. The form of the vibrations was of course altogether lost. Any simple musical tone, consisting of a regular succession of uniform vibrations, or any series of such tones, could, however, be reproduced with the greatest accuracy.

The next important step in the progress of invention was obviously the discovery of some means whereby the proper amplitude of each vibration, or succession of vibrations, either simple or compound, could be correctly reproduced by means of the electric current; and, when this was once done, the general problem of harmonic telegraphy may be said to have been solved. This having been accomplished, it was not difficult to foresee that two important practical applications might be expected to follow, namely, multiple transmission and vocal transmission. I believe that this discovery of the true method of transmitting composite vibrations was first publicly announced in the Journal of this Society,* in a paper contributed by Mr. Elisha Gray, it having been made by him in December, 1874. It consists in causing the effective strength of the electric current, by which the transmission is effected, to rise

* Gray, *Journ. of Am. Elect. Soc.* vol. i. p. 13. This apparatus and its mode of operation will be found described in detail in Gray's Patents, No. 1,874, of May 4, 1876 (Great Britain), and 186,340, of Jan. 16, 1877 (United States).

and fall with the varying amplitude of the vibrations or waves which are to be reproduced. Nothing could be more simple and beautiful in a theoretical point of view, but the practical exemplification of the method, as is usual in such cases, presented considerable difficulty.

At the time of making this important improvement, Mr. Gray had already been engaged for more than a year in endeavouring to devise a practical means of transmitting and simultaneously reproducing a number of tones, so as to utilise them for the purpose of multiple telegraphy. Let us briefly glance at what he had already accomplished.

It was observed in 1837, by Dr. Page,* that a musical sound was produced by a magnet, between the poles of which a flat spiral was placed. The sound was heard whenever contact was made or broken between the coil and the battery. These observations were confirmed and extended by De la Rive,† Wertheim,‡ and many others. The apparatus employed by these experimenters may be described in general terms as an electro-magnet with a self-interrupting break-piece attached to its armature, and another magnet in the same circuit for producing the sounds. The sounds proceed from the core of the magnet itself, and are caused by the molecular change which takes place in the iron at the moment of magnetisation or demagnetisation. When the current is interrupted a sufficient number of times per second, the successive sounds produce upon the ear the effect of a musical note. The method by which Gray at first sought to accomplish the desired result of multiple transmission was by arranging two or more self-interrupting magnets, adjusted to different rates of vibration, so as to close the circuit of the same line at the sending station, while at the receiving station all the currents passed through a series of electro-magnets, equal in number to the transmitters, and having armatures severally adjusted to their respective rates of vibration. As Mr. Gray has already described this apparatus at length in a preceding number

* Page, *Am. Jour. Sci.* (1st series), vol. xxxii. p. 396. *Ibid.* vol. xxxiii. p. 188.

† De la Rive, *Traité d'Electricité, théorique et appliqué* (English translation, by C. V. Walker, vol. i. p. 300); also, *Knight's Mech. Dict.*, art. *Telephone*.

‡ *Ibid.* vol. i. p. 307.

of the Journal,* I need not enter into further particulars concerning its construction and arrangement, but will in a few words point out the reason why it failed to answer its intended purpose, except to a very limited extent. Suppose we have two self-interrupting transmitters, one of which, a, makes six vibrations in the same time that the other one, b, makes five. If we now set them in operation, first one and then the other, and record the pulsations on chemical paper at the receiving station, we should obtain the results shown in fig. 8 at a and b; but, if both are set in operation simultaneously, we get the result shown in the third line of the figure, at c. Now, it is obviously quite possible, by insuring a proper relation between the times of vibration of two or even more transmitters, to avoid any material interference between the different sets of pulsations; but a limit is very quickly reached, because, as you will readily perceive, any considerable number of transmitters, acting in this manner to open and close the same circuit, would produce a continuous current, and no analysis of the separate sets of vibrations at the receiving station would be possible.

I will now proceed to describe in general terms the nature of the improvement by means of which Mr. Gray was enabled to transmit an indefinite number of different series of vibrations without destroying their individuality. The details of his system, and the particular application of it to multiple telegraphy, having been already made known in a preceding number of the Journal,† I shall not attempt to enter into them at any length.

The strength of current in any circuit may be varied in two ways: by employing a constant electromotive force, and varying the resistance of the circuit, or else by varying the electromotive force, and allowing the resistance to remain constant. Gray employed the latter process in his method of multiple telegraphy. Each series of vibrations at the transmitting station, when added

* Gray, *Jour. Am. Elect. Soc.* vol. i. pp. 5, 6. For details and fuller description see Specifications of Gray's Patents. viz., 2,646, of July 29, 1874, and 974, of March 16, 1875 (Great Britain); also No. 166,095, of July 27, 1875 (U.S.); also *Knight's Mech. Dict.* art. *Telephone*.

† Gray, *Jour. Am. Elect. Soc.*, vol. i. pp. 13 *et seq.* Also see patents of Great Britain and United States, referred to in previous note.

to the existing ones by the depression of its proper key, carried with it its own section of battery, and, therefore, its electromotive

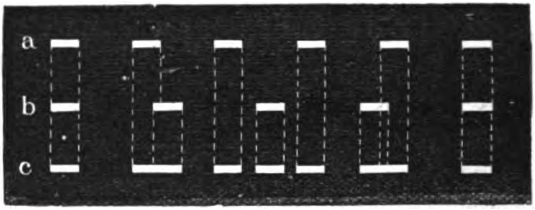


Fig. 8.

force was superposed upon that already in the circuit. The effect of this was to produce a resultant current of varying strength, which would be properly represented by a curve identical with that representing the resultant of the several sets of simple vibrations at the sending station. The analysis of the composite vibrations at the receiving station was effected by a series of electro-magnets, the several armatures of which were bars or plates adjusted to a certain rate of vibration, the normal rate of each armature bar differing from that of the others. Each armature bar will respond to its corresponding set of vibrations only, and it makes no difference whatever whether these vibrations are transmitted alone, or whether they form a constituent part of a composite series of vibrations. Each set of vibrations is broken up into dots and dashes by the action of a key, just as if it was an ordinary continuous current. But, as a matter of fact, the main circuit is never broken, although the strength of the current is constantly varied. The manner in which these armatures are thrown into vibration by the properly-timed impulses of the electric current acting upon the electro-magnet is, as you will readily perceive, strictly analogous to that of the swing, which can only be set in action by properly-timed impulses; or that of the tuning-fork, set in vibration by the tiny blows of the little atmospheric waves, in the manner which has already been explained.

The reproduction of articulate vocal sounds at a distance depends upon precisely the same fundamental principle as multiple harmonic transmission, namely, the transmission of composite vibrations. This will become evident from a consideration of the character of

articulate sounds, such as those of the human voice. The analysis of vocal sounds was first accomplished by Helmholtz.* It would occupy too much space to detail the experiments by which he succeeded in establishing the fact that the different vowel sounds are produced by the presence of a fundamental note, mingled with higher harmonics in various proportions, a harmonic tone being a weak or partial tone, caused by a rate of vibration twice, three times, four times, and so on, greater than that of the fundamental. The several vowels belong to the class of sustained tones which can be used in music, while the character of consonants mainly depends upon brief and transient noises. The problem in this case was to reproduce at the receiving station precisely the same vibrations in the atmosphere as those produced by the voice of the speaker at the transmitting station. We have seen why Reis was unable to accomplish this. Let us see wherein later inventors and discoverers have been more fortunate.

Some time prior to February, 1876, Gray conceived the idea of attaching to a stretched membrane, such as that used by Reis, a resistance apparatus, which should be placed in a constant circuit, and caused to vary with the vibrations of the membrane in response to the sonorous waves traversing the atmosphere and impinging upon it. Of course, if this could be done, it would be easy to attach an electro-magnet with an armature formed of a circular plate, which would respond to vibrations of every character, and thus reconvert the waves of electricity into aerial sound-waves. A caveat, describing this invention, was filed by Gray in February 1876, and he and others have since been engaged in perfecting and elaborating it, with a very satisfactory degree of practical success.†

* Helmholtz, *Die Lehre von dem Tonempfindungen* (Ellis's translation) chap. iii.

† Since the above was written, Mr. Thomas A. Edison, of Menlo Park, New Jersey, is said to have obtained very satisfactory results with a telephone constructed upon the general plan set forth in Gray's caveat, *i.e.* a variable resistance controlled by the vibrations of a diaphragm. Edison made the discovery that plumbago possessed the curious property of altering its electrical resistance in proportion to the pressure to which it is subjected, and availed himself of this discovery in the construction of his telephone. More recently the same experimenter is said to have obtained still better results by the use of carbon in the form of lampblack, from the smoke of an ordinary hydro-carbon lamp, compressed into a cylindrical button. For details of Edison's Telephone, see *Scribner's Monthly*, April, 1878, art. *Telephone and Phonograph*, by G. B. Prescott.

We will now turn to the labours of another inventor in the same field, Mr. Alexander Graham Bell. Like Gray, he had been for some time at work upon the problem of multiple telegraphic transmission by means of harmonic vibrations, and when we consider that each of them appears to have been, at least as late as October 1874, in entire ignorance of the labours of the other, the singular coincidence in the results which they finally attained was not a little remarkable. Gray had approached the subject from the stand-point of an electrician. Bell, on the other hand, was a physiologist, and so approached it from a different direction.* As early as 1867 he became interested in the researches of Helmholtz, because of their bearing upon the subject of his professional study, vocal physiology, or in other words, the mechanism of human speech. His earliest experiments appear to have been made in Boston in 1872, but were substantially repetitions of those already made by Helmholtz. In November, 1873, he completed an experimental instrument with two self-interrupting transmitting reeds, and two corresponding receiving reeds, the transmitters being connected in multiple arc, exactly as in Gray's first method. For reasons which have already been given in speaking of Gray's apparatus, it is possible to transmit two separate series of vibrations without material interference in this manner, yet a limit is very soon reached, because the current becomes practically continuous. Bell continued his experiments in multiple transmission during the years 1874 and 1875, but it does not appear that anything of practical importance in that direction resulted from them. At length he seems to have turned his attenion to the development of the speaking telephone, and in the spring of 1876 he arrived at some important results. In a communication presented to the American Academy of Arts and Sciences, May 10, 1876, and published in the Proceedings of the Society,† Mr. Bell gives a somewhat detailed account of his researches in telephony up to that date. I quote from this paper the following description of an experiment in vocal transmission, probably the first one in any

* See Paper, read by Professor Bell before the Society of Telegraphic Engineers, an abstract of which may be found in the *Telegraphic Journal*, vol. v. p. 276.

† Bell, *Proc. of Am. Acad. of Arts and Sciences*, vol. xii. p. 1.

degree successful, which appears to have been made by him early in the spring of 1876, and is of great interest:

"Two single-pole electro-magnets, each having a resistance of ten ohms, were arranged upon a circuit with a battery of five carbon elements. The total resistance of the circuit, exclusive of the battery, was about twenty-five ohms. Drumheads of gold-beater's skin, seven centimetres in diameter, were placed in front of each electro-magnet, and a circular piece of clock-spring, one centimetre in diameter, was glued to the middle of each membrane. The telephones, so constructed, were placed in different rooms. One was retained in the experimental room and the other taken to the basement of an adjoining house. Upon singing into the telephone, the tones of the voice were reproduced by the instrument in the distant room. When two persons sang simultaneously into the instrument, two notes were emitted simultaneously by the telephone in the other house. A friend was sent into the adjoining building to note the effect produced by articulate speech. I placed the membrane of the telephone near my mouth, and uttered the sentence: 'Do you understand what I say?' Presently an answer was returned through the instrument in my hand. Articulate words proceeded from the clock-spring attached to the membrane, and I heard the sentence: 'Yes, I understand you perfectly.' The articulation was somewhat muffled and indistinct, although in this case it was intelligible. Familiar quotations were generally understood after a few repetitions. The effects were not sufficiently distinct to admit of sustained conversation through the wire. Indeed, as a general rule, the articulation was unintelligible, excepting when familiar sentences were employed. Occasionally, however, a sentence would come out with such startling distinctness as to render it difficult to believe the speaker was not close at hand."*

There is reason to suppose that Bell had formed some idea of the possibility of this result as early as 1874, although its practical exemplification does not appear to have taken place until shortly before the date of the paper from which the above extract is taken. It will be observed that his method differs from that of Gray,

* Bell, *Proc. of Am. Acad. of Arts and Sciences*, vol. xii. p. 7. See also *Tel. Jour.* vol. v. p. 277.

inasmuch as the latter varies the resistance in the circuit without changing the electromotive force, while Bell varied the electromotive force, the resistance remaining constant. The battery current served no other purpose in Bell's experiment than to permanently magnetize the soft iron cores of the electro-magnet, while the magneto-inductive waves generated by the movements of the armature were superposed upon this current. In September 1876 Professor A. E. Dolbear substituted a permanent steel magnet for the electro-magnetic arrangement previously employed by Bell,* and the instrument thus improved is now going into very extensive use. Its articulation, while distinct, is not very loud, although sufficiently so in a well-constructed instrument to admit of lengthy sustained conversations, without the slightest misunderstanding or repetition. Of course, it is not to be expected that the loudness of this form of telephone can be increased very greatly beyond its present volume, for we can at best only get from it the mechanical equivalent of the human voice, deducting the loss inseparable from its conversion, first into mechanical motion, then into electricity, then into magnetism, and, finally, back again into motion. The most striking results are to be looked for in the direction first pointed out by Mr. Gray, for the reason that, if an effectual method of controlling the resistance of the circuit by means of atmospheric vibrations can be discovered, the source of power, which in this case is the battery, may be augmented to any required extent. It is not to be denied that the problem thus presented is one of exceeding mechanical difficulty; but there is no reason to suppose that it may not be successfully solved. It is to the development of this variety of the speaking telephone, rather than to that of the magneto instrument, that inventors will find it most advantageous to turn their attention, for I hazard little in saying that the latter has already reached such a surprising degree of efficiency as to leave comparatively little more to be done within the necessary limitations which have been pointed out.

* Dolbear, *The Telephone*, p. 119. (See also preface of same work.)

EDISON'S MICRO-TASIMETER.*

[Extracted from "THE SCIENTIFIC AMERICAN" of June 22, 1878.]

The latest of Edison's inventions, and perhaps the most interesting to physicists, is his Micro-tasimeter, or measurer of infinitesimal pressure.

The thermopile, hitherto foremost among delicate indicators of changes of temperature, must now be consigned to the rear ranks, and the radiometer, which exhibits the motive power of the most subtle of forces, must retire in favour of an instrument that can weigh that force.

The micro-tasimeter is the outcome of Professor Edison's experiments with his carbon telephone. Having experimented with diaphragms of various thicknesses, he ascertained that the best results were secured by using the thicker diaphragms. At this stage he experienced a new difficulty. So sensitive was the carbon button to changes of condition, that the expansion of the rubber telephone handle rendered the instrument inarticulate, and finally inoperative. Iron handles were substituted with a similar result, but with the additional feature of musical and creaky tones distinctly audible in the receiving instrument. These sounds Professor Edison attributed to the movement of the molecules of iron among themselves during expansion. He calls them "molecular music." To avoid these disturbances in the telephone, the handle was dispensed with; but it had done a great service in revealing the extreme sensitiveness of the carbon button, and this discovery opened the way for the invention of the new and wonderful instrument.

The micro-tasimeter is represented in perspective in figs. 1 and 2, in section in fig. 3, and the plan upon which it is arranged in the electric circuit is shown in fig. 4.

The instrument consists essentially in a rigid iron frame for holding the carbon button, which is placed between two platinum surfaces, one of which is fixed and the other movable, and in a

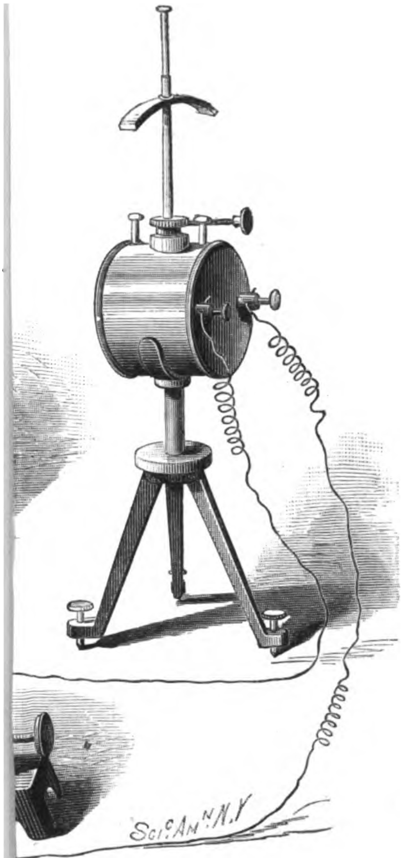
* The Society is indebted to Mr. Edison for the blocks of the illustrations of this article.

device for holding the object to be tested, so that the pressure resulting from the expansion of the object acts upon the carbon button.

Two stout posts, A, B, project from the rigid base piece, C. A vulcanite disc, D, is secured to the post, A, by the platinum-headed screw, E, the head of which rests in the bottom of a shallow circular cavity in the centre of the disc. In this cavity, and in contact with the head of the screw, E, the carbon button, F, is placed. Upon the outer face of the button there is a disc of platinum foil, which is in electrical communication with the battery. A metallic cup, G, is placed in contact with the platinum disc to receive one end of the strip of whatever material is employed to operate the instrument.

The post, B, is about four inches from the post, A, and contains a screw-acted follower, H, that carries a cup, I, between which and the cup, G, is placed a strip of any substance whose expansibility it is desired to exhibit. The post, A, is in electrical communication with a galvanometer, and the galvanometer is connected with the battery. The strip of the substance to be tested is put under a small initial pressure, which deflects the galvanometer needle a few degrees from the neutral point. When the needle comes to rest, its position is noted. The slightest subsequent expansion or contraction of the strip will be indicated by the movement of the galvanometer needle. A thin strip of hard rubber, placed in the instrument, exhibits extreme sensitiveness, being expanded by heat from the hand, so as to move through several degrees the needle of a very ordinary galvanometer, which is not affected in the slightest degree by a thermopile facing and near a red hot iron. The hand, in this experiment, is held a few inches from the rubber strip. A strip of mica is sensibly affected by the heat of the hand, and a strip of gelatine, placed in the instrument, is instantly expanded by moisture from a dampened piece of paper held two or three inches away.

For these experiments the instrument is arranged as in fig. 2, but for more delicate operations it is connected with a Thomson's reflecting galvanometer, and the current is regulated by a Wheatstone's bridge and a rheostat, so that the resistance on both sides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of the galvanometer is equal, and the light pencil from the reflector falls on 0° of the scale. This arrangement is shown in fig. 1, and the principle is illustrated by the diagram, fig. 4. Here the galvanometer is at *g*, and the instrument which is at *i* is adjusted, say, for example, to ten ohms resistance. At *a*, *b*, and *c* the resistance is the same. An increase or diminution of the pressure on the carbon button by an infinitesimal expansion or contraction of the substance under test is indicated on the scale of the galvanometer.

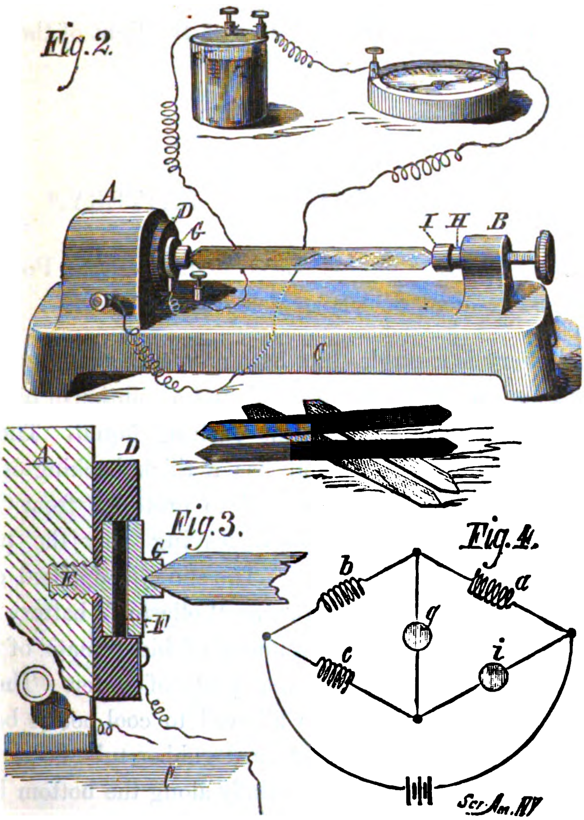

Fig. 2. Fig. 3. Fig. 4.

The carbon button may be compared to a valve, for, when it is compressed in the slightest degree, its electrical conductivity is increased, and when it is allowed to expand it partly loses its conducting power.

The heat from the hand, held 6 or 8 inches from a strip of vulcanite placed in the instrument—when arranged as last described—is sufficient to deflect the galvanometer mirror so as to throw the light-beam completely off the scale. A cold body placed near the vulcanite strip will carry the light-beam in the opposite direction.

Pressure that is inappreciable and undiscoverable by other means is distinctly indicated by this instrument.

Professor Edison proposes to make application of the principle of this instrument to numberless purposes, among which are delicate thermometers, barometers, and hygrometers. He expects to indicate the heat of the stars and to weigh the light of the sun.

BYRNE'S PNEUMATIC BATTERY.*

By William Henry Preece, Electrician, General Post Office, and Vice-President Society of Telegraph Engineers.

1. This battery is a single-fluid battery with zinc for the positive metal, platinum for the negative metal, and a solution of bichromate of potash and sulphuric acid for the exciting liquid. The zinc is amalgamated. The platinum foil has a thick backing of copper which is covered with thin sheet lead to protect it from the acid, and which is then well varnished, leaving only the platinum foil opposed to the zinc (fig. 2). The zinc is opposed on each side by platinum in the form known as Wollaston's battery (fig. 4). The solution is composed of 12 ounces of bichromate of potash, one pint of sulphuric acid, and five pints of water. The salt is dissolved in warm water and then allowed to cool before being inserted in the cell. A thin lead or hard rubber tube descends into the cell (fig. 3), and extends horizontally along the bottom between the opposing plates, and is perforated along the horizontal portion

* Paper read before the Physical Society. The construction of the battery is shown in figs. 1, 2, 3, 4 (for which the Society is indebted to the Proprietors of "Engineering"), and was fully described by Mr. Preece at the meeting of the Society of Telegraph Engineers of Feb. 27. See "Journal," vol. vii. p. 60.

so as to allow a current of air which is pumped through it by a syringe or hand-pump (fig. 1) to pass through the liquid and agitate

Fig. 2.

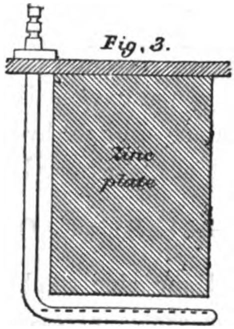

Fig. 3.

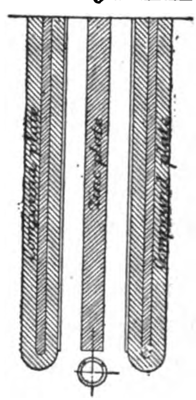

Fig. 4.

it violently. The cell is of ebonite or hard rubber, and the arrangement of the plates is such that when not in action they can

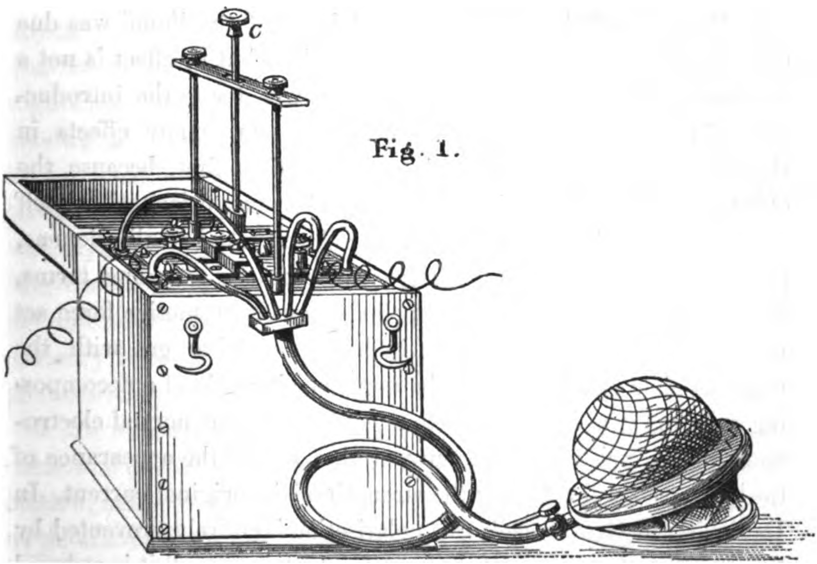

Fig. 1.

be lifted and maintained out of the liquid. The ten cells exhibited were constructed by Mr. Ladd.

2. Solutions of bichromate of potash as exciting agents in batteries

are becoming very common. This salt was suggested for the purpose by Poggendorff in 1839. His mixture consisted of

$K_2 Cr_2 O_7$ - - -	3 parts
So_3 - - - -	4 ,,
Water - - -	18 ,,

In this solution some of the oxygen is held by a very weak affinity, and it readily combines with the hydrogen evolved upon the negative plate. The action of the battery is probably as follows :

$$3\,Zn + K_2\,Cr_2O_7 + 7\,H_2\,So_4 = 3\,Zn\,So_4 + \overbrace{Cr_2\,O_3\,3\,So_3 + K_2\,So_4} + 7\,H_2O.$$

The resulting compound being zinc sulphate, chrome-alum, and water. The result is a remarkable increase of electromotive force. We apparently not only have the electromotive force due to the contact of zinc and sulphuric acid, but an equal force acting in the same direction due to the contact of the platinum and bichromate. Hence the electromotive force present is about twice that of a Daniell's cell. It is really 2·028—Daniell's cell being 1.

3. Poggendorff thought that the efficiency of the liquid was due to its depolarising effect on the negative plate, but its effect is not a restoration of power, but an addition of power due to the introduction of a fresh electromotive force. There are many effects in electricity improperly attributed to polarisation, first, because the effects are not understood, and secondly, because polarisation itself is not understood, and, as it has been well remarked in all sciences, physicists are apt to hide their ignorance in loud sounding terms. Polarisation is a term properly applied to an electromotive force set up by the contact of nascent hydrogen or other gas with the negative plate of a battery or the negative electrode of a decomposing cell, and it acts in a direction contrary to the normal electromotive force producing the current that causes the appearance of the hydrogen, and reducing the strength of the original current. In Daniell's and Grove's cells this polarisation is entirely prevented by the instant reduction of the hydrogen. In Smee's cell it is reduced by the mechanical removal of the hydrogen, but in Grove's cell and in the numerous applications of Poggendorff's solution we have not only polarisation prevented, but a fresh electromotive force set up,

acting in the same direction as the normal electromotive force. The action of these batteries is strongly corroborative of the modern contact theory supported by Helmholtz and Thomson.

4. Poggendorff used zinc in diluted sulphuric acid and carbon in his bichromate solution, the two being separated by a porous cell. Now it is observed in such a battery that the current resulting from it rapidly weakens when the circuit is completed. This effect is generally said to be due to "polarisation;" but it is thought by the author to be due to increase of internal resistance. Many efforts have been made to remedy this defect. Mechanical agitation of the liquid was found to be beneficial, and as early as 1857 Grenet suggested and patented a mode of rotating and moving the plates themselves, of agitating the fluid by mechanical means and by pumping air through it, and of forcing the fluid itself to flow through the cells so as to be constantly renovated. By this means he produced very powerful batteries. Trouvé, Chutaux, Camacho, Fuller, and others have followed in the same direction, but no one has produced such a powerful battery as Dr. Byrne, of Brooklyn, New York. A stout platinum wire 32 inches long of No. 14 B.W.G. (·089 inch in diameter) was gradually brought to a glowing red heat, which ebbed and flowed with the cessation or renewal of the air flow. A brilliant electric light was maintained between two carbon points which similarly varied in intensity with the flow of air. Mr. W. Spottiswoode's large induction coil was worked to its utmost limit, giving full intense sparks 18 inches in length with the air flow, and only 8 inches without air. Indeed all the effects which are ordinarily produced by seventy or eighty ordinary Grove's, were easily repeated with these ten cells when pneumatically agitated.

5. To what is this great increased strength of current due? It cannot be due to depolarisation, for polarisation means first deterioration, and depolarisation means restoration of power. There is no deterioration or restoration, but an absolute accession of power. It must, therefore, be due either to an increment in the electromotive force, or to a reduction in the resistance of the circuit conveying the current. Now, Ohm's law shows that the strength of the current (C) varies directly as the electromotive force (E)

producing it, and inversely as the resistance (R) opposed to its flow, or

$$C = \frac{E}{R}.$$

If R remains constant any increase in E will increase C in the same ratio, and if E remains constant, then, as we gradually reduce R, C must increase until, when we make $R = 0$, C must become infinitely great, or $C = \frac{E}{0} = \infty$. Hence in any battery the gradual reduction of R offers a ready means to increase the strength of the current flowing.

Of course there must always be some resistance present, and, therefore, the current can never become infinitely great, but, if the resistance external to the battery be so small as to be neglected, then the reduction of the internal resistance offers a readier means to increase the current than any possible increase of electromotive force.

6. Is the increased strength of current in Byrne's battery due to an increase of electromotive force or to a reduction in internal resistance? I have tested the electromotive force of the battery by various methods, at different times, when it is idle, when it is at work, when air was being pumped into it, or had been pumped into it, and invariably the electromotive force has been perfectly constant. Hence the increased strength observed must be due to diminished resistance.

7. This diminution is effected in two ways; first, the peculiar compound construction of the negative plate reduces the normal resistance of the cell to a minimum; and, secondly, the flow of air produces an abnormal reduction due either to mechanical, chemical, or thermal causes. The influence of the compound negative plate is clearly shown in experiment 13*b*.

8. Now the first effect observed is an extraordinary evolution of heat in the cells. If the battery be retained in action long enough, the liquid will boil. To what is this heat due?

a. It is not evident when the battery is idle, but it appears at once when action commences, and when the air is made to flow. Hence it is due either to the air or to the current.

b. If two cells be taken, one being short-circuited and the other disconnected, heat appears in each cell when air is made to flow. Hence it is not a sequence of the current but is due to the air.

c. If four cells be taken and filled with solution,

1. Complete but disconnected,
2. With zinc but without platinum plates,
3. With platinum but without zinc plate,
4. Without any plates at all,

and the air be turned on, then heat will commence at once to be developed in Nos. 1 and 2, but nothing beyond the small increment of heat due to friction will be observable in Nos. 3 and 4. In 14 minutes No. 1 had risen to 168° Fahr. and No. 2 to 120° Fahr. Hence it is dependent on the presence of zinc.

d. No. 1 cell disconnected was cooled down to 84° and observed for five minutes; there was no increase of temperature; it was short-circuited, but no air turned on; and in fifteen minutes the temperature rose only to 95° assisted no doubt by the mechanical disturbance of the liquid. Hence the heat is due to the presence of air and of zinc.

e. A fresh and well-amalgamated zinc and an unamalgamated zinc were taken in separate cells and the heat generated observed. The heat in the amalgamated cell remained constant, while that in the unamalgamated one rose in four minutes to 110°. Hence it is evident that the heat produced is that due to the combustion of the zinc, and that the probable action of the air is to materially assist this combustion by rapidly renovating the acid in contact with the zinc and removing the freshly formed sulphate of zinc from the neighbourhood of the plate.

9. How far is the reduced resistance due to the direct action of the air, and how far to the direct action of heat?

If it be due to the air the effect mnst be either chemical or mechanical.

The chemical effect is *nil*, for—

a. Oxygen, hydrogen, and air were alternately pumped through the liquid, and no difference whatever was observed in the strength of the current produced.

b. The constancy of the electromotive force under all conditions

is a proof of the absence of any additional chemical reaction. Hence the action of the air must be purely mechanical.

10. What is the mechanical effect of the flow of air?

A cell was fitted with a well-amalgamated zinc and a carbon negative plate inclined to each other at an angle of 45°, the bottom edges being near to each other. The current was measured on a tangent galvanometer.

a. Air was pumped in between the two plates, and instantly the strength of the current was increased to its full limit.

b. The pump itself was filled with the bichromate solution, and the liquid itself, instead of air, was forced on the zinc plate alone. No additional effect was observed.

c. The liquid was pumped on the negative plate, an increased effect was at once observed without the evolution of heat.

d. On simply rubbing the surface of the carbon with a piece of wood the same effect was observed.

Hence the mechanical effect of the air is to replace the solution in contact with the negative plate with fresh liquid.

11. But what effect has this mechanical action of the air on the internal resistance of the cell?

a. A cell was fitted up with carbon electrodes, the resistance between them was 16 ohms. Air was turned on, and in three minutes it rose to 18 ohms, and remained constant at that figure whether air were blown through or not. Hence the air has a tendency rather to increase the resistance of the liquid than to diminish it when the liquid is used as an electrolyte only.

12. The cell was fitted up with zinc electrodes, its resistance fell at once to 1·5 ohms, and, on blowing air through, it fell to ·5 ohm.

Hence it is evident that, besides tending to produce heat and the rapid combustion of the zinc, the internal resistance of the liquid is materially reduced by the chemical affinities present when air is laid on.

13. What effect has heat alone on the internal resistance of the cell?

a. A glass flask was fitted up with a zinc and an ordinary platinum plate immersed in the bichromate solution, and heat was

applied beneath it by means of a spirit-lamp. The temperature, electromotive force, and internal resistance were simultaneously observed:

Temperature. Fahr.	E. M. F. Daniell's Cell = 1.	Resistance. ohm.
60°	1·961	·85
80	1·961	·77
100	1·970	·68
120	1·970	·61
140	1·970	·54
160	1·970	·50
180	1·970	·49
200	1·970	·45

At 210° the acid attacked the zinc with great violence, rendering further observations impossible.

b. The platinum plate in the same cell was replaced by a Byrne's compound copper-lead-platinum plate:

Temperature. Fahr.	E. M. F. Daniell's Cell = 1.	Resistance. ohm.
80°	1·73	·78
100	1·88	·61
120	1·92	·35
140	1·97	·24
160	1·97	·19
180	1·97	·17
200	1·97	·14

At 210° acid attacked the zinc with such violence as to prevent further observation.

Hence it is evident that the evolution of heat is accompanied by a considerable reduction in the internal resistance of the battery, and that this is favoured by the lower resistance of the compound negative plate.

14. To examine the action of high temperatures in promoting the combustion of zinc.

A small piece of of zinc was placed in a flask containing a small quantity of mercury, the flask was filled with the bichromate solution, which was heated until it boiled. This latter took place at 215° Fahr. but the zinc remained protected by the amalgamation.

A plate of amalgamated zinc immersed vertically in the solution became attacked, if not allowed to rest in the mercury, but when so attacked, if it was allowed to again rest in the mercury, after a short time the action ceased. This would seem to show that the hot solution had a silent solvent action on the zinc, even when amalgamated, but a much more energetic action, accompanied by a violent disengagement of gas, when the amalgam had worn off.

15. What influence has heat alone in modifying the internal resistance?

Two platinum plates were immersed in the solution, and the resistance between them measured at different temperatures:

Temperature. Fahr.	Resistance. ohms.
70°	1·78
80	1·64
90	1·50
100	1·42
110	1·37
120	1·24
130	1·15
140	1·08
150	1·00
160	·86
170	·79
180	·65
190	·43
200	·31
210	·20
215 (boiling)	·04 (variable)

Hence it is evident, that, as heat modifies the chemical affinity between the molecules of the solution, so it reduces the resistance.

16. It is, therefore, clear that the wonderful strength of current produced by this battery is due to the great reduction of its internal resistance, produced in the first place by the peculiar compound construction of the negative plate; in the second place, by the peculiar effect of the rapid flow of air through the cells; and in the third place, by the production of heat. The action of the air flow is principally mechanical, but by hastening the combustion of

the zinc it tends to generate heat, which of itself tends to reduce the resistance. The mechanical action of the air is to remove from the neighbourhood of the negative plate the chrome-alum which is formed there, and from the neighbourhood of the positive plate the zinc sulphate, and to bring in contact with both plates a constant supply of fresh solution.

The principal defect of the battery is the rapid consumption of zinc and reduction of the bichromate solution. The cell becomes saturated with zinc sulphate, and the bichromate of potash becomes reduced to that peculiar double salt chrome-alum. If it is worked hard, it has to be freshly charged whenever it is wanted. Nevertheless, it can always be kept ready for action, it is free from all noxious vapours, and it is certainly a most valuable addition to the physicist's laboratory. Dr. Byrne introduced it for cauterising purposes, and it is largely used for such operations in America.

[NOTE.—I have, since this was written, found that the application of a spirit lamp to any form of bichromate battery, especially Grenet's, considerably strengthens the current flowing, and intensifies its effects.—W. H. P.]

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. VII. 1878. No. 24.

The First Ordinary General Meeting of the Session (1878-79), and the Sixty-ninth Ordinary General Meeting of the Society, was held on Wednesday Evening, Nov., 13th, at the Institution of Civil Engineers, Great George Street, Westminster, Mr. W. H. PREECE, Vice-President, in the Chair.

It was announced that the following gentlemen had been transferred from the Class of Associates to that of Members, viz.—

Frank T. Talbot.
F. de Marsac.
W. P. Binney.

The Chairman called upon Mr. A. Jamieson to read his paper, "On Cable-Grappling and Cable Lifting."

CABLE GRAPPLING AND LIFTING.

BY ANDREW JAMIESON, A.I.C.E., M.S.T.E.

Before entering upon the main subject of this evening's paper, it may be as well to briefly analyse the chief causes of the interruption and breakage of submarine cables, with a view to guarding against their future failure. The chief causes may be enumerated as follows:—

1st. Total breakage due to abrasion, suspension between submarine eminences, insufficiency of slack, volcanic eruptions or terrestrial disturbances, or possibly large masses of rock becoming detached and falling across the cable in descent.

2nd. In shallow waters from ships' anchors.

3rd. Faults caused by teredos, or marine animal borers.

4th. Break of the conductor, with perfect or good insulation.

5th. Latent faults in the cable when laid, developing after time; deterioration of core, joints, or sheathing.

6th. Lightning.

Firstly. With regard to breakages due to abrasion: this cause is mainly attributable to the cable being swayed to and fro, under the influence of tides, currents, etc.; and where these are likely to occur, the only means of guarding against them is that of submerging the cable very carefully, so that it may suit itself to the configuration of the ground, and using a heavy type of sheathing. The fault of suspension is clearly due to insufficiency of slack; hence the great importance of a thorough knowledge of the ground over which a cable has to be laid.

Volcanic eruptions or terrestrial disturbances are actions over which we have no control, and for which at present no visible means of provision can be made, except that of using a heavy type of cable where they are to be apprehended.

Secondly. Cables are frequently damaged and broken by ships' anchors in shallow waters. This danger can only be lessened by using as heavy a type of cable as possible, and carefully marking off the ground by buoys or otherwise, and as far as possible avoiding anchorage.

Thirdly. The frequent injury to the core from the attacks of teredos, or other marine animals, may be guarded against by coating the core and servings with some anti-teredo substance which will either be fatal or distasteful to these obnoxious animals, and which at the same time may have no deteriorating effect upon the core or cable. Various means have been proposed with this object in view, such as covering the core with powdered glass, brass, or copper tape, and saturating the servings with andiroba oil.

Fourthly. Cases of interruption from a broken conductor with perfect or good insulation frequently occur, often in joints, but more frequently in the main conductor, due to an overstraining of the copper wires in manufacture, or in paying out, or perhaps brittleness of the copper, as has been reported in the case of

Hooper's core. It is evidently caused by the chemical action of the sulphur inherent in the second coating of Hooper's material, when not thoroughly eliminated, acting upon the conductor, rendering it brittle. The simple remedy for this kind of fault is to be found in taking great precaution to have the copper of the conductor thoroughly annealed, and capable of bearing a certain strain with elongation before breaking, and being careful not to put more strain upon the same when twisting it into a conductor than is absolutely necessary to form a good lay; so regulating the core and outer servings that they will take up any strain, to be afterwards put upon the cable before the conductor. In the case of Hooper's material, the remedy is to be found in eliminating or so insulating the sulphur, that it cannot act upon the conductor.

Fifthly. As regards provision against a latent fault in the cable when laid, developing itself after time, or a deterioration of the core, and joints or sheathing: we must look for a remedy in a thorough system of electrical and practical testing, during manufacture and coiling from factory to ship; and *too* much care cannot be taken in having each mile of core and cable thoroughly tested, as well as every joint and bundle of wire of which the cable is composed, for it is only by so doing that all the future annoyance and expense arising from latent faults may be avoided.

As regards the sixth cause, viz.: Lightning, the remedy may be looked for in a good system of lightning guards, and in carefully earthing the cable ends when an unusually severe thunderstorm occurs. Cases of interruption or fusing of a cable conductor are fortunately of infrequent occurrence, and I only know of four well-authenticated instances.

When from any cause a submarine cable is broken, or becomes too faulty for the transmission of messages, a repairing ship, fitted with all the necessary appliances for grappling and lifting the cable, is despatched with the least possible delay to the position localised by the electrician.

On arriving at the locality of the fault, or "cable ground," as it is technically termed, the first thing to be ascertained is the depth of water, and the nature of the bottom, with a general idea of its configuration, which is done by careful soundings. The

most approved apparatus for this purpose, up to the present time, is Sir William Thomson's wire sounding machine, a full description of which appeared in the Society's Journal, Vol. III., page 206, as well as in Mr. H. Benest's communication on soundings taken on the West Coast of South America, Vol. V., No. 19.

It has been found a great improvement to have the base of the sounding machine permanently fixed to the after rail, instead of running out and in the whole apparatus, when taking soundings.

In obtaining deep sea soundings, it has been found advantageous to slip the sounding weight, simply elevating the tube containing a specimen of the bottom. Sir William Thomson's tube and valve for recovering soundings sometimes fails in very deep water. A very neat, and at the same time a more simple plan has been devised by Mr. C. H. Phillips, Electrician, Eastern Telegraph Company, of which the following is a description:—

Looking at the annexed sketch, Fig. 1:—

1. Is the sounding line.
2. The sounding weight.
3. Tube with bell mouth, and closed at upper end.
4. A small collar fixed to tube (3) to prevent its passing upwards through the hole in the sounding weight.

5 & 5. Two small springs attached to tube (3) to prevent its dropping out during lowering.

6. A short line fixed to bottom of tube (3) and end of sounding line (1).

7 & 7. Sir William Thomson's disconnecting clip.

It will be easily seen that, immediately upon the sounding weight reaching the bottom, it becomes disconnected, and, upon raising the sounding line, the tube is drawn from the bottom of the weight, and at the same time scoops up a specimen of the bottom, retaining it until it is brought to the surface for inspection. The great advantages which this sounding machine possesses over the old method of sounding by line are (1st) speed and (2nd) accuracy. As regards speed, a sounding of 1,000 fathoms can easily be obtained by this machine in half-an-hour from the time of stopping the ship and letting go the lead to that of getting it up

again and starting ahead. Its accuracy arises from the fact that the wire is of so very small a diameter (0·03 in.) that it offers a minimum resistance to deflections by tides or currents.

So much of the after success or failure of grappling depends upon a knowledge of the depth and ground to be worked upon, that too much importance cannot be placed upon the necessity of obtaining accurate soundings. This precaution, especially in great depths, will well repay the extra time and trouble incurred; for even in waters apparently well surveyed, errors and omissions occur, and it often happens that where a cable has been laid, the chart indicates no bottom. It was only the other day that we went to repair a cable in the Levant, and the only sounding marked was 200 fathoms, no bottom; whereas, when we came to take soundings, we found at that place 800 fathoms, and within a few miles along the line of cable 1,300, which at once led us to surmise (as the test showed the fault to be here) that it would turn out to be a case of breakage from suspension, which was found to be the case. And it was also evident that those who laid the cable could have had no knowledge of the depth or bottom, for the cable had spanned a submarine valley of three or four miles.

During the operation of sounding, it is well to ascertain at the same time if any currents or tides exist, and the allowances to be made for these and winds during the future operation of grappling and lifting.

Soundings having been obtained, the next thing to do (if no good landmarks are available for taking accurate bearings) is to place a "mark buoy."

Mark buoys should be large, ride well, and with an easily distinguishable beacon fixed to the top of as tall a pole as the buoy will conveniently carry. "Bird cage" beacons are as a rule preferable to flags, from the frequent occurrence of the latter becoming entangled and wound round the staff, or not standing out well when the wind is light. Spherical silvered globes have been prepared, and no doubt would be easily distinguishable at great distances when the sun was bright, owing to the reflected rays, but they have not been practically adopted. At night, flags, beacons, or globes are of no avail, and we still lack a good plan of distin-

guishing mark buoys from sunset to sunrise. Lamps are used, but they necessitate the lowering and sending away of a boat's crew to fix them, which under the circumstances of heavy weather is impossible, and is always shunned if it can be avoided; besides, they are liable to become broken, upset, or extinguished. Up to the present time no thoroughly successful plan of fixing and keeping alight a lamp during the eight or ten hours of night has been devised. Certainly we have tried lamps swung in gimbals and otherwise; but the great inconvenience arising from fixing and removing them is a decided drawback. I have proposed, but, unfortunately, have not had an opportunity of yet practically trying, a lamp which would burn clearly for ten hours, supported and fixed on knife edges and gimbals, on the plan adopted by Sir William Thomson in his patent mariner's compass, which I think ought to answer well.

I have to-day (November 13th, 1878) got a letter from Sir James Anderson, which he has just received from Sir Richard Collinson, Deputy-Master, Trinity Board, in answer to certain enquiries he made about lighting buoys with gas, for cable repairing purposes, in which Sir Richard says that compressed gas has proved a complete success for lighting buoys.

He states that "Mr. Punshon, the agent of Pintsch's Patent Lighting Company, brought a buoy to the Blackwall Wharf, filled with gas compressed to ten atmospheres, and after burning for 22 days, day and night, without any diminution of power, it was put into the water, rolled about, played upon by a fire-engine, and satisfied the Trinity House Board. It was then refilled, and laid two cables south-west of the Mouse Light, where, after riding out some heavy gales of wind, he visited it, and found it burning so brightly that it was made out by the glass at four miles and by the naked eye at two miles." Certainly this is a great step in the right direction, and, if successfully carried out, will prove of great advantage to cable ships.

A good electric light, such as that now used in the navy and elsewhere, would assist in discovering a buoy at night; but I doubt whether it would be sufficiently handy to enable the ship to be kept up to within sight of a mark buoy on a rough, windy

night. I hope soon to have an opportunity of trying an electric machine in this way. It would therefore be a great boon conferred upon those who have to repair cables, as well as to the shareholders and others, if some convenient and efficient plan were devised of keeping a repairing vessel in position when on the ground at night, so that she might start work the first thing in the morning without delay.

Now, having ascertained the depth of water, nature of the bottom (if possible) and currents, winds, &c., and fixed the position of our mark buoy by careful observations, we are ready to commence grappling. But before describing the operation, it may be as well to give a general idea of the grapnel system, as at present used, with the appliances for working the same, discussing later on improvements or alterations which might be advantageously made.

Looking at the accompanying sketch, Fig. 2:—

1. Tank or platform for holding or supporting the grapnel rope.
2. Picking up gear.
3. Dynamometer.
4. Fair lead pulley.
5. Bow baulks and sheaves.
6. Grapnel rope.
7. Grappling chain.
8. Grapnel.

The grapnel (proper) is therefore attached to about 15 fathoms of $\frac{3}{4}$ or $\frac{7}{8}$ grapnel chain. The chain in turn is shackled to the grapnel rope, which passes over the bow and fair lead pulleys, underneath the dynamometer pulley, and with three, or if necessary, four turns round the picking-up drum, ending in a coil, placed in a suitably arranged tank or platform behind the picking-up gear. The drum is revolved by a steam engine and gearing, either directly or indirectly attached to its framing. It can also be stopped or eased by friction brakes, suitably fixed to the pulleys on one or other of the motion shafts. The dynamometer is generally placed midway between the picking-up drum and the fair lead

pulley, and indicates by a pointer and scale the strain brought to bear upon the grapnel rope, *e.g.*, when the grapnel is being drawn over the ground, or when elevating the cable. The fair lead pulley is simply a loose pulley, free to run upon a shaft and bearings, as well as to move longitudinally along the same, and consequently guide the grapnel rope from the bow to the dynamometer pulley, or *vice versa*. The bow sheave is the last guide which the grapnel rope receives on passing from or to the ship. The grapnel rope is composed of steel wires, each wire being served with hemp, varying in size and strength according to the nature of the ground, and depth of water. The grappling chain which is attached between the end of the grappling rope and the grapnel shank, is generally 15 to 20 fathoms long, and composed of ordinary $\frac{3}{4}$ inch or $\frac{7}{8}$ inch chain, and serves to protect the grapnel rope from becoming worn on the ground, as well as to keep the grapnel more or less well-buried, and up to its work when in action. The grapnel itself is of various forms and sizes, according to the nature of the ground to be worked upon. For instance, there are long toed, short toed, self-relieving toed, centipede chain, and other grapnels; but for the present let us select the common centipede grapnel, with a trail chain behind to act as a damper or preventive against jumping, and commence lowering it to the bottom of the sea preparatory to grappling. The grapnel rope is lowered by the picking-up gear, and as a Rotometer or length measurer is always attached, you can easily ascertain when the grapnel should be nearing the bottom. The ship should therefore be moved easy ahead, in order to prevent the chain and grapnel becoming jumbled in a heap, and care should be taken not to pay out rope in excess of the depth. When the grapnel and chain have reached the bottom, and a sufficient quantity of rope has been paid out to insure easy working without trailing the rope on the ground, the grapnel rope should be carefully parcelled or served with matting where it is likely to become chafed on the bow sheave, or fore-foot and side of the ship—a circumstance which is very likely to occur, if there is any swell or up and down motion of the ship.

The operation of grappling is now fairly commenced, having taken precautions that all necessary stoppers and appliances are at

hand, in the case of hooking cable, or the grapnel rope taking a run, under a sudden strain due to catching rocks. The ship should as far as possible be brought head to wind and tide, that is, working against any local forces, so that she may be the more easily checked in her forward movement, should anything occur to require this being done; or she may be allowed to drift stern first, only moving the engines when it is required to keep her up to the course.

The responsible telegraph engineer or officer in charge now sits or stands upon the grapnel rope, watching the strains by dynamometer, but mainly trusting to the indications felt by the nerves of his hands, or feet. He can easily distinguish in shallow waters between the gradually (I might say, softly) increasing strain, denoting "cable hooked," and the sudden, sharp, wicked strain, intimating "rocks engaged." But in deep water, or when working on stiff clay bottom, it requires great experience and judgment to correctly tell whether the cable has been hooked or the grapnel is simply labouring through heavy ground. This difficulty is of course increased with the depth, as the vibrations and strains imparted to the rope are lessened and the indications consequently deadened.

Rocks are a great source of trouble, and have caused no end of breakages to grapnels, ropes, &c., besides great waste of time. It is no uncommon occurrence for a repairing ship to have 3 or 4 grapnels rendered useless in a morning's work.

I shall have the pleasure of explaining and exhibiting, for your discussion and approval, a grapnel specially designed to overcome this difficulty.

In the meantime, let us suppose that we have been lucky in hooking the cable, and that we have commenced picking up. This is an operation which requires considerable experience, as the ship has to be carefully and expertly handled, so as not to bring any undue strain on the cable, as well as always to keep the grapnel rope vertical, or as nearly so as possible, in order to prevent the grapnel skidding or sliding along the cable. The speed of picking up should be so regulated that surging and jerking may be avoided. Many a cable has been broken during picking up by an

over anxiety to bring it smartly to the surface and not giving time for the ship to come to, or easing out rope when she fell off. Of course the ease with which a cable may be picked up greatly depends upon the amount of *slack* originally laid. For cables laid in waters up to 1,000 fms., it is most decidedly advisable to so arrange the slack according to the depth, that the cable may be picked up on the bight when the time comes for it to be repaired, and a safe rule would be to lay cables with 1 per cent. of slack for every 100 fms. of *depth*. When a cable has to be repaired in waters over 1000 fms., it is only under exceptionally favourable circumstances that we can expect to bring it up on the bight without breaking.

The cable has firstly to be hooked, lifted a certain height and buoyed, then elevated or eased by a ship some 3½ miles from the buoy, while another ship catches and brings the cable to the surface, between the buoy and ship. To obviate all this trouble or difficulty, grapnels have been devised for cutting one side and bringing up the other, which we shall more particularly refer to later on.

Assuming that the cable has been successfully elevated to the bows, it should be securely stoppered on each side of the grapnel nip to lengths of strong coir or grapnel rope, the one leading over the starboard and the other over the port-bow sheave. With THREE bow sheaves this can easily be done, the central one being occupied by the grapnel rope, and those on either side for heaving in or slacking off one or other of the ends of cable.

There are two plans at present in use for stoppering a cable at the bows, both of which necessitate the lowering of a man over the side. The one is that of simply nipping the cable on the bight of a chain and link, and is seen on the left hand of the annexed sketch, Fig. 3, and the other on the right, certainly the more secure although it may take longer time, is that of taking a rolling hitch and half-hitch with spun yarn seizings before and behind as well as through the last link of the chain stopper.

I think it might be quite possible to devise some means of clamp arrangement, whereby the loss of time and danger arising from stoppering the cable at the bows might be avoided, as well as

a plan of severing the cable when stoppered, without having to let a man over the side in a boatswain's chair to effect the same with file and saw.

The cable having been securely stoppered on each side of the grapnel nip it should be cut, and heaving in commenced by picking up drum on the one side while slacking out on the other.

This plan is preferable to that of buoying one end, as it permits of having both sides tested and a knowledge of the electrical condition of the cable being ascertained before picking up towards the fault. It enables this to be done without materially altering the ship's position.

If the good side be first brought on board it can of course be sealed, let go, and buoyed before testing the other, unless (as is sometimes advantageous) shore should be informed how much may be expected to pick up, &c.

Suppose that the good side has been buoyed, that is, its end securely fastened to a length of chain with mushroom anchor, and sufficient buoy rope, with riding and bridle chains, &c., between the same, and the buoy [Fig. 4], it is always best to let the cable down to the bottom when buoying an end (not so in the case of a bight), and to, therefore, depend entirely upon the mushroom, buoy rope, and chain for holding the buoy from drifting, insuring at the same time that the cable, when once down, bears no part of the strain.

Everything clear, it is now time to pick up towards the fault. This is done by steaming the ship gently forward over the line of cable, elevating and taking it in at the lowest possible strain by the picking up gear, and coiling the same in one or other of the cable tanks. Sometimes the cable as it leaves the ground gets jammed in a rock, and great care has to be taken in extricating it without overstraining or breaking.

It is always a special point of interest to those engaged in repairing a cable, and more especially to the electrician, to see and examine the fault. Care should be taken when the fault or broken end comes on board to avoid jamming or altering its form or condition in any way before it has been thoroughly examined

and tested, as every fault picked up has a history of its own, and tells a tale furnishing valuable experience.

All information regarding the fault should be collected, sifted, and arranged, in order to assist in dealing with and localising more correctly similar ones in future, and guarding against recurrence.

Of course, if a broken end come on board, or the cable get broken in picking up, the operation of grappling, hooking, and lifting the cable has to be repeated as just described, in order to secure the other end, and find that it tests all right.

Having secured it, and supposing that it tests all right, a joint and splice has to be made to the good cable on board which it is intended to lay in.

Jointing and splicing on board ship when out on a repair, have to be effected with the greatest possible expedition in order not to keep the ship hanging on to the cable longer than is absolutely necessary, and requires to be done by men specially trained for the purpose.

The process of jointing the conductor and (gutta-percha) core of a cable at sea is precisely the same as that described by Mr. George E. Preece in his paper on "Underground Telegraphs," and published in the Society's Journal, Vol. II., page 397. We need not therefore repeat it here, but simply consider that it has been well done. Preparatory to jointing, however, the sheathing and yarn servings have to be prepared and arranged according to the form of splice to be made. If the types of cable are of the same size and lay, a "factory splice" may be made, that is, the wires are inlaid; if of different types or lay, an "overlapping splice" will be best and quickest. In either case the iron wires on one side of where the joint is to be made have to be unstranded in *twos* and *threes* to a distance, say of 7 or 8 fathoms, bound there with spun yarn, and the core cut off within 3 or 4 feet of the fastening, while those on the other side are also firmly bound with spun yarn, but cut off square, leaving about 2 feet of core sticking out as seen by the annexed sketch, Fig. 5.

The yarn serving is neatly laid back and fixed to the iron

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sheathing on both sides, the two projecting ends of core being sufficient for the jointer to work upon without impeding him by the iron sheathing.

After the joint has been finished, cooled and tested, the yarn servings are drawn forward and neatly bound over the joint to protect it from the sheathing.

The iron wires which were unstranded and laid back, are now brought forward, and either alternately inserted in the place of those on the other side, being cut off at distances of 5 to 6 feet apart, making (what we have just mentioned) a "factory splice," or drawn over and laid round the sheathing on the opposite side. The sheathing is seasoned here and there with small soft iron wire, and the splice, whichever way it may have been done, is tightly served with spun yarn, and is just as strong as any other part of the cable of the same type when well done.

Paying out the new cable is now commenced, and continued until the buoy attached to the other end is reached, when the buoy with its moorings, &c., and the cable attached is lifted and brought on board. Both sides are now carefully tested, and if all right, a joint and splice are made between the two ends, and the bight carefully let go over the ship's side, tightening it as far as required before slipping.

There are almost as many precautions and observances to be attended to in paying out a few miles as a few hundred; but as a detailed description of these would occupy so much time, and properly belong to a paper on cable laying, we must omit them here.

All I shall say is this, that for paying out a short length (anything up to 10 miles) it is best to pay it over the bows, as it saves the necessity for passing the cable from stem to stern, or *vice versâ*, when the first end buoyed has been reached.

In fact, I am of opinion that it would be well if all repairing ships were fitted with break power on the same shaft as the picking up drum suitably arranged for paying out, in which case there would be no necessity whatever for paying out machinery aft, resulting in a saving of first cost and room, besides dispensing with

a lot of top weight, and confining cable work to the forward part of the ship.

Let us now turn our attentions to the different kinds of grapnels, ropes, &c., that have been devised for hooking and lifting submarine cables under different circumstances.

The first kind of grapnel used, and often used still, is that known as the long shanked five pronged grapnel of which Fig. 6 is a sketch. The prongs and shank are all rigidly welded together, the shank being about 4½ ft. long, with a swivel end.

Another and now more commonly used form, is that known as the "Centipede Grapnel," Fig. 7. It consists of a *square* bar of iron with a ring fixed at each end, and a series of double prongs wedged into holes in the shank. This form of grapnel has two or three advantages over the 5 pronged one. The prongs can be removed and others refixed on board ship without the difficulties of welding, it is easier to make, it affords a double chance of catching the cable (that is to say should the front toes miss, those behind may still catch the cable), and it can be furnished with a trail chain or weight attached to the after ring bolt, to prevent jumping.

Both these forms of grapnels, however, labour under the disadvantage that, if once engaged with rocks or other fixed obstacles, the greatest difficulty is experienced in relieving them without breaking the toes or some part of the grapnel system, either rendering grapnel useless for further work, and necessitating its being elevated on board ship, and a fresh one attached, lowering the same, and commencing the work over again; or, in the case of the grapnel rope giving way, losing part of the rope chain and grapnel. A kind of centipede may be used at a push, by employing a large chain (Fig. 8), with bars inserted in the centre of the eyes of the chain, as seen in the sketch. This proves a serviceable grapnel of the non-relieving type.

While repairing the Para-Pernambuco section in December, 1875, we experienced an immense deal of trouble from this cause, owing to the very hard rocky bottom there; and it occurred to me then, that a grapnel might be constructed with self-relieving toes, so

as to give way when engaged with rocks, but so constructed that the toes should automatically assume their normal working position immediately after slipping over the rocks. I mentioned my idea to Mr. Wm. F. King, Engineer-in-Chief to the Western and Brazilian Telegraph Company, *and we designed a form of grapnel to effect this object, On returning to England, I carried out several improvements, with the able assistance of Mr. Alexander Glegg, the maker, one of the Society's Associates, and have now got a practically efficient instrument.*

This grapnel permits of working on rocky or even ground without fear of sticking, at the same time affording every facility for catching the cable and retaining the same when caught, as well as preventing the sharp nip caused by ordinary grapnels, by having broad, well-rounded shoulders, upon which the cable may rest when being elevated to the surface.

Placed before you is a full-sized grapnel, ready for use, a model to exhibit the action, and a sectional drawing to explain the details.

In the accompanying drawing, Fig. 9 represents a sectional elevation, and Fig. 10 a corresponding plan of the improved grapnel. 1 is the shank of the grapnel; 2 is the cylindrical boss, which may be made of cast steel, malleable iron, bronze, cast iron, or other suitable material, and which contains and protects the spring 6; this spring may be constructed on the volute, spiral, or any other principle, and may be made of steel, india-rubber, or both combined—a volute spring being preferred. Round the lower end of the boss (2), project toes or prongs (8), which are cast or otherwise, formed solid with the boss, each pair of toes embracing a much longer toe (4), by preference composed of wrought iron or steel, which is held in position by the fulcrum pin 7, round which it is capable of revolving. The shank (1), which has attached to its upper end a shackle (10) provided with a swivel joint, is firmly screwed to the boss 2 by a long coarse pitched thread, secured by a jam nut (14) and clamp plate (5) in any desired position; the diameter of the shank is then reduced, as shewn at 15, and the reduced portion passes down through the

boss (2), and terminates in a screwed end (12), to which is attached the shackle (11) for the "trail" chain. 13 is a movable piston, capable of sliding up the reduced portion (15) of the shank (1).

The apparatus operates in the following manner:—The toes (4) when engaged by rocks or other obstacles are pressed outwards, and rotate round their respective fulcra (7), their inner ends (16) bearing and pressing against the movable piston (13), which travels up the reduced portion (15) of the shank (1), within the boss (2), compressing the spring (6); this movement may continue until the toes (4) move round to the angle shewn by dotted lines on the left hand side of Fig. 9, which position is amply sufficient to relieve the toes from any rock or other obstacle. As soon as the toes (4) are released, the piston is forced down under the action of the spring (6), and in its turn acts upon the toes at 16, thereby restoring them to their working angle; this angle is fixed by shoulders on the boss (2), which form stops between the boss and the toes.

By fixing the shank to the boss the toes (4) always maintain the working angle, unless engaged with rocks or other obstructions, and the initial tension may be increased or diminished by screwing down or up the shank or other adjusting nut or gland in the boss (2). The grapnel can be taken to pieces and put together again in twenty minutes.

This grapnel has already proved of good service in repairing cables in South America, under the skilful management of Mr. King, and has been adopted by the Eastern Telegraph Company, and other Companies are also making inquiries about it.

The saving in time alone, effected by not having to elevate the grapnel to the surface for inspection or renewal, will be found to more than repay its extra cost in a single expedition.

Grapnels are and have been used with provision for removing the toes when they become broken, but this necessitates their elevation to the surface every time a toe gets bent or broken, and it just takes as much time to insert a fresh toe as it does to attach a new grapnel, the only gain being a saving of expense in fixing the toes.

The following is an extract from a letter by Captain Richardson, s.s. "Chiltern," to Mr. Sherlock, chief engineer, s.s. "John Pender," and sent to me by the latter:—

Aden, June 14th, 1878.

My dear Sherlock,—Jamieson's grapnel has proved a great success.

I have never used any other, since here, while on the same ground (Red Sea) in 1876 we used to damage the grapnels at the rate of 8 or 9 a day.

Jamieson's patent has brought the cable up every time, and never been lifted unless it was with the cable on it.—I am, &c.,

A. Richardson.

I understand he also expressed himself equally well satisfied to Sir James Anderson after the expiry of the expedition.

For very deep sea work, where the depth and corresponding slack will not permit of the cable being elevated on the bight, owing to the strain, &c., grapnels have been specially designed, as I mentioned before, for cutting one side and holding on to the other until it is brought to the surface.

I understand Mr. Latimer Clark was the first to propose this novel plan. Perhaps he may kindly tell us all about his experiments and endeavours to devise a cutting and holding grapnel.

Mr. Lambert took up and worked out the idea; and, later on, Messrs. Johnson and Phillips.

These designs have all the same object in view, and it would be very interesting to hear from the respective authors a description of their several plans with an account of their trials.

We have, I think, now spoken of all the kinds of grapnels commonly in use, except to say that sometimes the cable is brought up upon the shackle pin. I have seen this occur three different times, and it was only the other day, 15th February last, that we brought up the cable, from 800 fathoms water, on the shackle pin next the grapnel. This arises from placing the shackle with the curved part towards the grapnel rope instead of the other way, when it could not possibly catch the cable.

There is nothing particular to remark about the grapnel chain. It simply requires to be of good material, with well made free and easy working links that won't jam or become entangled, and being of $\frac{3}{4}$ or $\frac{7}{8}$ inch iron leaves a large margin of safety strength

over that of the grapnel rope used. There are two plans of fixing, the one shown by plan A, the other by B. B has the disadvantage of a projecting eye, which is apt to foul with the rope when passing over the drum, and requires to be bound to the body of the shackle with wire seizing, which runs a chance of becoming loose or worn through on the ground, allowing the bolt to turn and slip out. Instead of wire fastening, the bolt in plan A is secured by a set screw, and the bolt itself screwed home by screwdriver, which necessitates a T screwdriver to be always at hand when it is required to fix or unloose this form of shackle. Upon the whole, plan A is preferable to B, as it presents no obstacles to passing clear. Grapnel ropes as fitted in various lengths, 200, 100, 50 and 25 fathoms; at one end of each length is spliced an oblong link and thimble, at the other end a thimble swivel and link. The object of the swivel is to relieve the rope from kinking and taking turns in it. The reason for having the grapnel rope in stated lengths is to facilitate uniformity, and admit of knowing the length out without referring to the Rotometer for buoying purposes, &c.

The grapnel rope most commonly used is that known as 4 by 4. It consists of 16 steel wires, No. 13 Birmingham Gauge, each wire being served with 5 or 7 well tarred manilla strands, four of these manilla served wires being stranded together, and the 4 strands laid up into one rope. This rope should bear a strain of 15 tons without breaking, and be of the very best material, but as a rule it is not considered safe to tax its strength beyond 10 tons, and I have seen it go at 5 tons. 6 by 6 (that is a rope made of 36 steel wires and manilla strands in exactly the same way as the 4 by 4, with 6 strands instead of 4) is sometimes used in shallow waters, with the centipede or fixed toed grapnel, where heavy and sudden strains, as well as chafing, are expected, but it is not required with the self-relieving grapnel, and from its great weight is impracticable in deep water 6 by 3 is a good rope, and coming into favour. 3 by 3 is too light and weak for ordinary grappling, and is only used for buoy rope. *White or untarred manilla* does not answer well when laid up with steel wires to form a grapnel rope. It absorbs water and tends to kink, coiling badly after use, causing great trouble in paying it out a second time, besides, its

co-efficient of friction is small, and it slips on the drum of the picking up gear.

When grappling in deep water, say over 1,000 fathoms, steel wire-manilla ropes are disadvantageous, owing to their weight. A 4 by 4 rope, working in 2,000 fathoms, raises the normal or working strain on the dynamometer between 65 and 70 cwt., lessening the sensitiveness of feeling and observation. I understand that pure manilla was successfully used last summer and the summer before by the Telegraph Construction and Maintenance Company in the Atlantic repairs, and that with 500 fathoms of 6 by 3 steel wire-manilla next the grapnel, and 2,000 fathoms manilla rope, the normal strain was only from 40 to 50 cwt., where, had 6 by 3 been used throughout, the normal strain would have been nearly 80 cwt. This certainly is a great improvement, as it affords much greater facilities for ascertaining when the cable is hooked, besides permitting of buoying the bight on an emergency with a buoy of smaller capacity, and lessening the working strain of the whole grapnel system. As a comparison between the two systems, we might cite that of bait and fly fishing. In the one case we have a dull, heavy line, with lead sinker, in the other, a fine, flexible, and light but strong cast, giving warning of the slightest nibble. Another strong proof of the advantages of the manilla rope for deep-water work is, that no rope was lost in the late Atlantic repairs, although the strain sometimes reached 9 tons. Some further details of this manilla rope and its behaviour, as well as the best mode of handling it, would be most instructive and interesting from those who used it. Lightness combined with sufficient strength is a matter of great importance, and the manilla rope bids fair to be the grapnel rope of the future for deep-water work.

Since writing this paper, we had occasion the other day to grapple for an old and weak cable in 100 to 150 fathom water, and the advantage of working with manilla was very marked indeed. You could tell so much sooner when the cable was hooked, and there was less fear of walking through it.

Every repairing ship should be furnished with three bow sheaves, the centre one for grappling, and those on either side for

paying out or picking up, as the case may be, when the cable has been brought to the bows. Two are bad enough, and one is a nuisance, from the time spent in fixing ropes and stoppers.

The bow baulks should stand well out beyond the bows of the ship, allowing the grapnel rope to clear the forefoot without heaving in the rope or backing the ship when it is required to change her course. They should be covered in flush between the iron girders with wood, and be supplied with two or three sets of stopper-hooks, in order to admit of relieving the strain from the picking-up gear when it is required to change the rope or shackle on a new length. This reminds me that the fewer the shackles employed in a grapnel rope the better, provided the whole length is of the standard strength, as every time a shackle passes the bow baulks, dynamometer, and picking-up gear, care has to be taken that it does not foul.

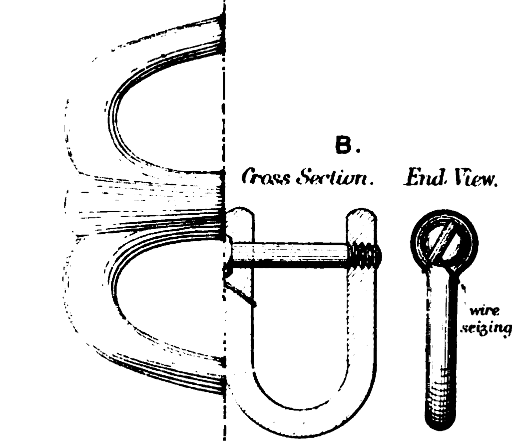
The dynamometer at present in use consists of a piston-rod, with piston, working in a bored cylinder, filled with water or oil (Fig. 11). The cylinder has a small pipe communicating with the top and bottom, to permit of the liquid passing below or above the piston as it rises or falls with the strain. To the top of the piston-rod is fixed a crosshead-guide working between vertical parallel columns, and to this guide is attached the dynamometer pulley, which runs loose on its axle. Provision is generally made so that a number of extra weights may be attached to the piston rod below the guide, but outside the cylinder, to increase the apparent sensitiveness of the dynamometer, when working in deep water or where heavy strains occur. The piston-rod head or guide carries a pointer which indicates on a scale (suitably marked in cwts.) attached to the framing of the dynamometer the strain brought to bear on the grapnel rope or cable.

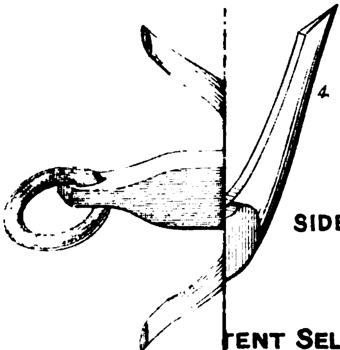
The scale may be marked off by calculation according to the following formula, but should also be verified by a practical test, as the rule does not take friction into account.

Referring to the above diagram, Fig. 12, let BC indicate the centre line of dynamometer placed midway between centres of the picking-up drum A and fair lead pulley D.

B.

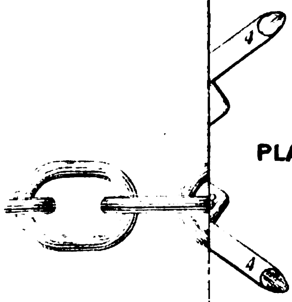
Cross Section. *End View.*





SIDE ELEVATION.

TENT SELF RELIEVING
GRAPNEL.



PLAN.

Let W = weight of dynamometer pulley, crosshead, &c., in cwts.

S = strain on cable or rope (*to be found*) in cwts.

BC = deflection in inches of rope or cable at point C from straight line between A and D.

AB = distance in inches between centre lines of picking-up drum and dynamometer.

Then completing the parallelogram of forces, that is drawing AE parallel to CD, and DE parallel to CA, and projecting CB to E, we see at once that CA represents the strain S, to the same scale that CE represent the resultant or W.

$\therefore$ CE : C A : : W : S; but $CE = 2\,BC$ and $CA = \sqrt{AB^2 + BC^2}$

$\therefore$ substituting $2\,BC : \sqrt{AB^2 + BC^2} :: W : S$

$$\text{or S (strain on rope)} = \frac{W\,\sqrt{AB^2+BC^2}.}{2\,BC.}$$

It will also be seen from the formula that the strains and deflections of the dynamometer are in inverse ratio to one another, so nearly that having marked the scale for one strain, the position of the others may at once be found without working out the whole formula for each strain or deflection.

The foregoing form of dynamometer gives a fair idea of the strain, when the grapnel rope or cable is *static*, that is not being picked up or payed out, but it does not account for speed or rate of these operations.

We still lack a dynamometer which embodies the foot-pound unit, or one pound lifted through one foot in one second. The one in use, however, does very well for grappling, but is not a satisfactory indication of the strain brought upon a cable when paying out.

I have designed a small instrument similar to that employed in taking diagrams of steam engines, to be kept in the testing room for indicating by a curve the strain at any moment brought upon the cable when being paid out. It consists of a cylinder revolved at a uniform rate by clock work, with a roll of metallic paper wound upon it, marked off to seconds and minutes. A pencil connected with the crosshead of the ordinary dynamometer moves up and down in unison with the same, and traces on the paper a curve as it moves round. Another pencil in connection with the

rotometer marks off on the curve the proper ratio and speed, in feet or fathoms, at which the cable is being paid out, so that a diagram may be taken of the strains at any time during paying out and kept for reference.

The picking up gear has received very little improvement within the last ten or fifteen years, although there are several deficiencies or apparent drawbacks in its arrangement and manufacture. It consists of a pair of horizontal steam engines with fast and slow gearing revolving a drum round which the cable is coiled three or four times, having break gear attached to the seccnd motion, worked by screw and handle.

In the first place it is very heavy and clumsy, and stands much in the way of the work, and would be handier if the engines were placed on the main deck instead of as at present on the upper deck, and only the drums and break wheel, &c., above. Secondly, the gearing is not sufficiently protected from fouling the cable. Thirdly, there is no provision made for automatically acting breaks, to permit of long lengths of cables being paid out over the bows. Fourthly, it should have two overhanging drums, with means of paying out one side while packing up the other when the cable has been cut, instead of having recourse to the steam winch, capstand, or bullards, and admit of putting on or taking off the grapnel rope or cable at any moment, without having to wait until a shackle passes or cutting the cable. Fifthly, steam of sufficient pressure and quantity should be supplied from a separate boiler, and not be dependent upon the main boilers of the ship. Sixthly, all handles for starting and stopping the gear should be brought to one platform, and so arranged that they may be manipulated with ease and without loss of time. Seventhly, a perfect system of telegraph communication should be fitted between the gear, bow baulks, and tanks, so that the attendant may not be continually on the strain for a shout or whistle. All these are requirements which the picking up gear of a repairing ship should have, but which unfortunately they don't as a rule possess.

I am confident of having omitted much useful information from want of time and fear of prolonging the paper, which I hope may be brought forward in the discussion.

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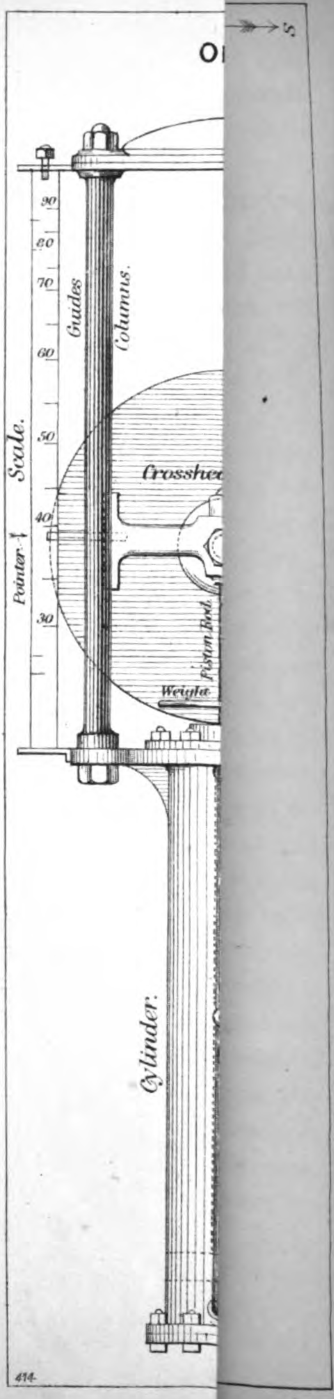


NOTE BY C. HOCKIN, M.A.

Mr. Jamieson has asked me to add a note as to the retarding strains to be applied when paying out a cable, to give any required amount of slack.

The principle on which the strains must be calculated has been so fully discussed in Vol. V. of this Journal, that it is unnecessary to refer again to it.

Beaufoy has found that the resistance to the passage of solid bodies through water, due to pressure on the surfaces perpendicular to the direction of motion, varies as the velocity raised to the power 2·1, and the resistance due to friction on surfaces parallel to the motion of the body is proportional to the velocity raised to the power 1.93.

Assuming this law, and putting p for the co-efficients of friction perpendicular to the line of cable, q for the co-efficient parallel to it, and w for the weight in water, each per unit length, v for velocity of ship, $v\,(1+n)$ for velocity of cable, and a the angle of inclination of cable to horizon, then

$$p\,(v \sin. a)^{2\cdot 1} = w \cos. a$$

and tension per unit of depth of water

$$= w - \frac{q}{\sin. a}\left(v\,(1+n) - v \cos. a\right)^{1\cdot 93.}$$

In practice we may with sufficient accuracy write

$$\sin. a = \left(\frac{w}{p}\right)^{\frac{1}{2\cdot 1}} \frac{1}{v}\left(1 - \frac{1}{4\cdot 2}\,\frac{w}{p}\right)$$

By measurements of the angles at which the French Atlantic cable was observed to be inclined to the horizon when paid out at various speeds, I found the most probable value of p to be 19·8 cwt. per thousand fathoms for a cable one inch in diameter, the unit velocity being one knot per hour. For q with the same units, the most probable value was 2·28. The value of q will depend on the degree of smoothness of the cable. The value given by Beaufoy for the friction of water against a surface of planed wood would make $q = 0{\cdot}174$. It is probable that q is larger than this for all cables. The lowest value of q that I have found is $q = 1{\cdot}00$. This value is calculated from the results of paying out a cable coated with Johnson & Phillip's tape and compounded, the surface being very smooth.

The following table may be found useful. Col. I. gives the ratio of the weight in water of 1,000 fathoms of cable in cwts. to its diameter in inches. Col. II. the corresponding value of a. Col. III. log sin. a. Cols. IV. to XI. the value of $\frac{1}{\sin a}\,(v(1+n) - v \cos.)\, a^{1\cdot 93}$. For $v = 5$ knots per hour, and different values of a and n. To use this table for any cable, find the weight of 1,000 fathoms of the cable in water in cwts., and divide the number by the diameter of the cable in inches, look for the nearest number in Col. 1, and against it is the value of a. Then the retarding strain to be applied to the cable to pay out any required degree of slack, at the rate of 5 knots per hour, will be fonnd thus: multiply the proper co-efficients found in Cols. IV. to XI. by the diameter of the cable in inches, and the product by the proper value of q, subtract the result from the weight of 1,000 fathoms of the cable in water expressed in cwts., and finally multiply the result by the depth of water expressed in 1,000 fathoms, the result is in cwts. For other speeds of the ship the co-efficients in Cols. IV. to XI. must be multiplied by $\left(\frac{v}{5}\right)^{1\cdot 93}$

TABLE.

$\frac{W}{D}$	a.			Log. Sine a.	5 %	6 %	7 %	8 %	9 %	10 %	11 %	12 %
I.	II.			III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.
	°	′	″									
2·61	4	35	40	$\bar{2}$·90363	·9692	1·351	1·794	2·297	2·860	3·482	4·162	4·901
3·16	5	1	50	$\bar{2}$·94291	·9047	1·257	1·664	2·128	2·645	3·216	3·621	4·519
3·76	5	27	50	$\bar{2}$·97873	·8572	1·186	1·565	1·996	2·476	3.006	3·585	4·218
4·41	5	53	47	$\bar{1}$·01167	·8172	1·126	1·431	1·884	2·338	2·828	3·368	3·954
5·11	6	19	35	$\bar{1}$·04215	·7778	1·063	1·402	1·780	2·201	2·665	3·172	3·720
5·87	6	45	20	$\bar{1}$·07050	·7540	1·030	1·347	1·705	2·104	2·542	3·021	3·533
6·68	7	10	56	$\bar{1}$·09699	·7287	·9915	1·293	1·633	2·011	2·426	2·879	3·370
7·54	7	36	28	$\bar{1}$·12185	·7113	·9632	1·252	1·576	1·987	2·338	2·764	3·231
8·45	8	1	55	$\bar{1}$·14528	·6963	·9383	1.215	1·526	1·871	2·250	2 662	3·107
9·42	8	27	16	$\bar{1}$·16740	·6832	·9166	1·183	1·482	1·812	2·175	2·570	2·997
10·44	8	52	35	$\bar{1}$·18837	·6760	·9022	1·160	1·443	1·767	2·117	2·496	2·906
11·51	9	17	48	$\bar{1}$·20829	·6700	·8897	1.143	1·418	1·726	2·064	2·430	2·825
12·63	9	42	55	$\bar{1}$·22725	·6651	·8788	1·121	1·391	1·690	2·016	2·370	2·751
13·80	10	7	55	$\bar{1}$·24535	·6611	·8694	1·105	1·368	1·657	1·978	2·316	2·685
15·03	10	33	0	$\bar{1}$·26266	·6579	·8612	1·091	1·346	1·627	1·934	2·267	2·624
16·31	10	57	54	$\bar{1}$·27922	·6591	·8584	1 083	1·332	1 606	1·905	2·229	2·576
17·64	11	21	8	$\bar{1}$·29512	·6609	·8563	1·076	1·320	1·588	1·879	2·194	2·532
19·02	11	49	10	$\bar{1}$·31039	·6667	·8592	1·075	1·314	1·576	1·861	2·169	2·499
20·46	12	12	13	$\bar{1}$·32508	·6692	·8584	1·070	1·305	1·561	1·840	2·140	2·463
21·94	12	36	52	$\bar{1}$·33922	·6756	·8623	1·071	1·301	1·553	1·828	2·120	2·436
23·48	13	1	25	$\bar{1}$·35286	·6822	·8565	1·072	1·298	1·546	1·814	2·102	2·411
24·00	13	19	25	$\bar{1}$·36257	·6876	·8704	1·074	1·298	1·542	1·806	2·091	2·395
26·00	13	50	56	$\bar{1}$·37905	·6957	·8756	1·075	1·295	1·534	1·792	2·070	2·367
28·00	14	21	18	$\bar{1}$·39433	·7082	·8861	1·083	1·299	1·534	1·788	2·060	2·351
30·00	14	51	46	$\bar{1}$·40909	·7207	·8968	1·091	1·286	1·535	1·784	2·051	2·336
32·00	15	19	0	$\bar{1}$·42184	·7359	·9109	1·104	1.314	1·542	1·788	2·051	2·332
34·00	15	46	26	$\bar{1}$·43433	·7476	·9211	1·112	1·320	1·545	1·787	2·047	2·323
36·00	16	13	10	$\bar{1}$·44610	·7634	·9359	1·125	1·331	1·554	1·793	2·049	2·322
38·00	16	39	10	$\bar{1}$·45724	·7796	·9514	1·140	1·344	1·565	1·802	2·055	2·323
40·00	17	4	30	$\bar{1}$·16780	·7932	·9640	1·151	1·354	1·572	1·807	2·057	2·322

The CHAIRMAN next called upon the Secretary to read a short communication from Mr. Francis Lambert, Member, on the same subject.

ON GRAPNELS FOR RAISING SUBMARINE CABLES IN DEEP WATER.

By FRANCIS LAMBERT.

After the attempt made by Sir Samuel Canning to recover the Atlantic Cable in 1865, when he succeeded in raising the bight about 1,200 fathoms by means of a grapnel lowered from the S.S. "Great Eastern" in a depth of 2,000 fathoms, a discussion arose amongst Telegraph Engineers as to the best means of recovering a cable from such a great depth. Several plans were proposed, and the one which met with the greatest approval was the employment of two or more ships to grapple the cable in different places at the same time, so as to obviate the strain which arises when the bight of a cable is raised by one grapnel only. It was further proposed that one ship should employ a cutting grapnel to sever the cable while the bight was held suspended by the other ship, or buoyed in order that one end might be raised to the surface without subjecting the cable to any great strain. The latter plan was carried out in 1866 by Sir Samuel Canning, when the 1865 cable was successfully repaired.

Another plan, that proposed by Mr. Latimer Clark, was to employ a grapnel which should securely grip the cable and then sever it in two, one end only being held fast and raised to the surface. Mr. Clark invented an apparatus to effect this object, a description of which was published in the Mechanics' Magazine of the 20th July, 1866.

It is obvious that if the operation of lifting a deep sea cable could be performed in the manner proposed by Mr. Clark, repairs could be carried out with greater facility, and a very considerable saving of expense would be effected, as the employment of more than one ship would be unnecessary.

Having this idea in view, I gave some attention to the subject, and devised the form of grapnel hereinafter described.

Before the details of the apparatus were decided upon, some experiments were carried out to test the efficiency of the plan I proposed for obtaining a firm grip of the broken end of cable, which I will here describe, as they may be interesting to the Society.

Two pairs of cast iron cams or eccentric jaws, A and A′, Fig. 1[a], hollowed out in the inner faces to partially surround the cable, were mounted to revolve freely on four strong steel studs firmly attached to the cast iron plate B. The pair of cams A, nearest the broken end of cable, were made of rather greater radius than the pair A′, so that they might grip the cable somewhat in advance of the latter pair, and so take part of the strain. The radius of the pair A′ ranged from 2·8 inches to 3·5 inches, the length of the faces being 7 inches.

This apparatus being fixed in the testing machine, and the piece of cable subjected to a gradually increasing strain in the direction indicated in the sketch, the cams closed on the cable and held it, without any appreciable slip occurring until the strain reached above 5 tons, when the cable parted between the jaws A′.

Fig. 2[a] shows the position of the cams immediately before the cable parted.

Taking an average of three experiments, the strain at which the cable broke between the jaws was 5 tons 3 cwt.

Various forms of cams were tried, but those I have described, the inner faces of which were hollowed out and serrated, gave the best results.

These experiments were made at the works of Messrs. Brown, Lennox & Co., by means of their chain cable testing machine—the cable used being a piece of the 1865 Atlantic Cable, which had an average breaking strain of about 6·5 tons.

The results of these preliminary experiments being most satisfactory, I submitted them, together with a rough working model to show the form of grapnel I had devised, to the late Admiral Sherard Osborn, C.B., Managing Director of the Telegraph Construction and Maintenance Company, who approved of the idea, and authorised the construction of a grapnel on the proposed plan,

for trial on the repairs of the Lisbon and Madeira Cable, which were then pending.

The following is a description of the apparatus constructed, a rough working model of which, together with a wooden model (half size) of another form of the grapnel, in which only one pair of cams is used for holding the end of cable, I have submitted for the examination of Members.

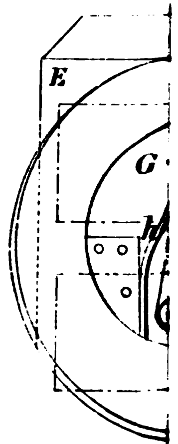
In the accompanying diagrams, Fig. 1, sheet 1, shews the grapnel in elevation. Fig. 2 is a side elevation of the same, and Fig. 3 is a plan with the box covers removed. Figs. 4, 5, and 6, sheet 2, are detail views of the movable platforms and eccentric jaws, and the devices for holding them in position.

As shewn in Figs. 1 and 2 the apparatus somewhat resembles an anchor in form, being made with a shank A, furnished with a stock C, to ensure its dragging in the proper position for grappling the cable, and having two pairs of prongs D D, each pair converging and meeting at the points to make a sort of skeleton fluke. The shank is forked at the bottom, and spreads out into two pairs of curved arms A′ A′ on each side, the whole being forged into one with the shank, and strengthened by a tie bolt B, Fig. 1, passing through the arms A′ and through short lengths of iron pipe B′, placed between the arms, which are thus braced rigidly together, and so form frames to hold the gripping and cutting devices. The prongs are fitted with taper sockets D′ D′ on the outside arms and secured by nuts, so as to be readily detached and replaced when damaged. E E are guide boxes bolted, one pair between each pair of arms A′. As both sides of the grapnel are fitted alike, it will be sufficient to describe the fittings on one side only.

The boxes, E, which, as well as their fittings, are made of gun metal, are rectangular, and are open at the bottom and closed at the top by covers. Each box has a vertical slot, F, at each side, to admit the cable, and allow it to pass down across the box, as shewn in Fig. 1, to bring the strain across the arms, A′. Within each box a platform, G, is fitted, to slide up and down, the top surface of each pair of platforms being curved to correspond with the curve formed by the bight of the cable when resting thereon. H H are pairs of eccentric jaws, the edges of which are grooved

and serrated to obtain a good bite of the cable, on each platform, pivoted on steel bolts, I I, the jaws being provided with springs, K, to cause them to close together, as shown in dotted lines, Fig. 6. The two pairs of jaws close in the same direction to hold the same piece of cable, and in order that they more effectually support one another, the pair nearest the severed end is made of rather greater radius than the other pair, so that they may close on and grip the cable somewhat in advance of the other pair. Each pair of jaws is held open by a detent, C, standing up through a slot in the platform between them. This detent is jointed to a lever, and on the under side of the platform, and kept up by a spring, *e*, this is released by the pressure of the cable itself, on a stud, *f*, also jointed to the lever, *d*, and projecting through the platforms, G, but to a greater height than the detent, so that the jaws will be released and close on the cable immediately it comes between them.

Fig. 4, sheet 2, is a section on line 1, 2, Fig. 1, to show the method of supporting the platform in their guide boxes; *g g* are spring catches, a pair of which is provided on box E, at the inner end, to support the platform, G. These catches work on pivots, *g'*, one at either side of the slot, and are pressed together by springs, *h*, to support a stud, *i*, on the end of the platform, G. The stud projects through the slot, and rests upon the upper end of the catches, *g*, which are inclined, so that when the bight of cable is raised a certain distance from the bottom, the weight coming on the platform will force the catches, *g*, apart, and allow the platform to slide down in the boxes, as shewn in dotted lines, Figs. 1 and 4. These catches work within the curve of the inner arm, A', of each pair, so that they are protected from receiving injury whilst dredging: *z z*, Figs. 2 and 3, are a pair of V cutters, fixed against the outer end of one of the guide boxes, E, and partly covering the bottom of the slot, F, so that as the platform yields under the weight of the cable, the latter will be guided between the cutters and severed. The boxes, E, are each closed at the top, partly by a fixed cover and partly by a movable sloping shutter, L, pivoted at K, and kept closed by a spring. These shutters cover the entrance to the slots, F, and are provided with wings at the sides to cover the top part of the slot. The sloping top of the shutter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serves to guide the cable to the entrance of the slots, the shutters being so pivoted as to be oscillated, as shewn by dotted lines, Fig. 2, under pressure of the cable, upon that part which covers the entrance to the slots. It will be seen that the arrangement of these shutters is such as to effectually prevent the eccentrics being choked by stones or mud, as they are so pivoted as to be only opened by such pressure as will be caused by the cable when hooked, whilst any pressure coming on them while dredging will tend to keep them closed.

This grapnel was tried on the repairs of the Lisbon and Madeira cable in 1874, and although the experiments were unattended with complete success, the results were very promising, and showed that with a little modification in the holding arrangement the apparatus would perform the work for which it was designed.

It will be seen by the following table of strains, during a dredging operation made in a depth of about 2,100 fathoms, that the bight of the cable was cut, and one end raised 1,750 fathoms by means of this grapnel;—

MAY 18, 1874. S.S. "AFRICA."

Time.	Rope overboard.	Rope picked up.	Average indicated strain.	Strain on Grapnel.	Difference of strain on Grapnel each 100 fathoms.	
H. M. p.m.	Fms.	Fms.	Cwt.	Cwt.	Cwt.	
4 15	...	...	...	...	...	*Let go Grapnel*— Lat. 36° 27′ 15″ N. Long. 12 29 40 W.
...	1,400	...	...	...	...	
...	1,800	...	66·9	...	...	
...	1,900	...	73·3	...	...	
...	2,000	...	76·2	...	...	
...	2,050	...	80·3*	...	...	* Grapnel on bottom. Sounding about 2,100 fathoms. Engine ahead.
...	2,100	...	66·1	...	...	
...	2,200	...	68·3	...	...	
...	2,300	...	67·6	...	..	
...	2,400	...	68·0	...	...	
5 40	2,500	...	72·2	..	...	Stopped paying out rope.
6 0	...	...	74·9	...	...	Ship put on course E. true. Engine dead slow.
6 15	...	...	74·8	...	...	
6 30	...	...	83·3	...	...	
6 45	...	...	93·6	...	...	
7 0	...	...	99·2	...	...	
7 15	...	...	97·3	...	...	
7 30	...	...	97·9	...	...	
7 45	...	...	99·0	...	...	
8 0	...	...	103·5	...	...	
8 15	...	...	107·3	...	...	
8 30	...	...	108·0	...	...	
8 45	...	...	...	...	...	
9 0	...	...	109·3	...	...	
9 15	...	...	118·0	...	...	Steady strain.
9 20	...	...	...	...	...	Engines stopped a few minutes.
9 26	...	...	107·0	...	...	Do. ahead again.
9 40	.	...	...	...	...	Engines stopped. *Commenced picking up*—Lat. 36° 27′ 15″ N. Long. 12 24 5 W.
10 5	2,400	100	98·8	...	...	
10 24	2,300	200	103·5 ?	...	...	
10 40	2,200	300	98·0	...	...	
11 0	2,100	400	94·6	...	...	
11 13	2,000	500	92·2	...	...	
11 27	1,900	600	83·8*	16·4	...	Bight probably cut. Cable no doubt partly severed by cutters, whilst dragging out bight, before commencing to pick up.
11 40	1,800	700	85·4	20·6	+4·2	
11 55	1,700	800	84·0	21·8	+1·2	
May 19. a.m.						
0 15	1,600	900	82·2	22·6	+0·8	
0 30	1,500	1,000	76·6	19·6	—3·0	
0 45	1,400	1,100	75·0	20·6	+1·0	
1 2	1,300	1,200	77·6	25·8	+5·2	
1 20	1,200	1,300	74·8	25·6	—0·2	
1 40	1,100	1,400	73·4	26·8	+1·2	
1 55	1,000	1,500	74·2	30·2	+3·4	
2 16	900	1,600	72·9	31·5	+1·3	
2 32	800	1,700	73·6	34·8	+3·3	
2 50	700	1,800	72·2	36·0	+1·2	
3 8	600	1,900	72·2	38·6	+2·6	
3 28	500	2,000	71·1	40·1	+1·5	
3 50	400	2,100	66·1	37·7	—2·4	
3 55	350	2,150	27·0	...	...	Strain suddenly fell to weight of rope on Grapnel. End of Cable lost.

The strains given above Col. 3 are the means of several observations recorded as each 100 fathoms of rope was picked up, the gear being stopped at the time for that purpose; and it will be seen by the numbers in Col. 5 that the average increase of strain on the grapnel as each 100 fathoms of rope was recovered was 1·42 cwt. The actual weight of 100 fathoms of cable in water being about 1·5 cwt., the agreement in the numbers is sufficiently close to show that one end of the cable was being raised.

Upon examination of the grapnel, it was evident that the bight of the cable had been severed in the cutters, and that the end had been fairly gripped by the eccentric jaws. The reason of the end slipping was no doubt due to the external compound on the cable acting as a lubricant.

I may here remark, that the preliminary experiments before referred to were made with cable which had no external compound: I am therefore of opinion that the holding power of this apparatus would be effectual for any cable of the latter description.

The promising results obtained on this trial induced me to give further attention to the subject, with a view to modifying the holding devices of the grapnel, so as to obtain a longer grip of the cable, and for this purpose I devised an apparatus in which the holding power is obtained by means of sliding wedges, instead of eccentric jaws.

I have had no opportunity of trying this plan as yet, and therefore cannot speak as to its efficiency, but it is well known that a cable can be very firmly gripped by means of sliding wedges, and that such means are used to hold a cable when testing its breaking strain.

The manner in which I propose applying the wedges in a grapnel will be seen by the following description of the apparatus:—

Fig. 1, sheet 3, is a plan, Fig. 2 a front elevation, and Fig. 3 a side elevation of the grapnel.

Figs. 4 and 4^a, sheet 4, show the position of the jaws and platform when the cable is cut, and one of the severed ends retained by the jaws.

Fig. 5 shows a cross section of the sliding jaws and the guides in which they move, taken on line 1 — 1, Fig. 2.

Fig. 6 shows a separate view of the sliding stop for retaining the jaws.

The frame of the grapnel is constructed somewhat like that in the apparatus before described, consisting of a shank, A, bifurcated at the lower end, the two arms spreading out and united at the bottom by a cross-piece, A′, on either side of which are curved arms, B, to carry the holding devices. C are curved prongs fixed in sockets in arms B, and united at points.

The holding devices being alike on both sides of the grapnel, it will be sufficient to describe one side only. DD are a pair of wedge-like sliding blocks, forming the holding jaws: they are fitted to slide between a pair of guides EE, inclined towards one another, and forming a groove or way between them, narrower at one end than the other, so that when the sliding jaws are situated at the wide end of the groove, as in Fig. 1, the cable will just go between them; and when at the narrow end, as in Fig. 4, their faces will be nearly touching. The sliding jaws D are wedge-shaped, their outer surfaces being inclined to correspond to the inclination of the guides E, and their faces, *a a*, parallel to one another in the direction of their length, so as to grip the cable equally at all parts.

These faces *a a* are, however, slightly inclined towards one another in the downward direction, as shown in Fig. 5, so as to form a V-shaped space between them, into which the cable drops until it is tightly wedged therein.

The jaws are made of iron or steel, and their surfaces which are in contact with the guides E, are faced with gun metal, to diminish the friction. Their faces, which come in contact with the cable, are serrated to obtain a firmer hold.

The guides E, with the descending platform F, of which they form part, are made of steel, and are of sufficient strength to resist the great bursting strain to which they are subjected by the sliding jaws becoming wedged under the weight of the cable. This platform F is mounted to oscillate on a horizontal axis G, as shown; this axis, passed through the part A′ of the frame, and through one of the curved arms B, and firmly secured, is a bolt of sufficient strength to sustain the weight of the cable.

The guides E are provided with overhanging flanges *e*, to keep the jaws D in the groove, the jaws being retained at the widest end of the groove by a stop. This stop consists of a vertically sliding block *f*, Figs. 1, 2, 3, 5, and 6, received in a mortise in the platform F, extending downwards, as shown, to form a guide for it. In setting the wedge jaws, this stop is raised between them, in which position it is held by springs *g g*. The stop is so formed as to prevent any movement of the wedge jaws until the cable coming upon it forces it down out of the way. H is an arm pivoted at one end to the frame A′, and hooked at the other end to take into a notch in the end of the platform F, in order to hold the latter in the tilted position shown in Fig. 2, whilst dredging for the cable. The arm is maintained by a spring I, so as to yield when the weight of the cable comes on the platform, and allow the latter to fall so as to bring the bight of cable between the cutters K K, fixed to the arm B of the grapnel frame, whereby the cable is severed. The action of the grapnel will be seen in Fig. 4.

When the cable is severed, as above mentioned, the part which is between the jaws drags the latter along to the narrower end of the groove E, in which the jaws with the cable between them become tightly wedged, whereupon the end can be raised to the surface. The platform F being pivoted as described, assumes the position shown in Figs. 4 and 4^a, whereby a direct drag is obtained, and any liability of chafing the cable on the arm of the frame is avoided.

To prevent the wedges and groove becoming choked by stones or mud, a box or cover *l* is fixed upon and encloses the top of the platform F and guides E. This cover is provided with a longitudinal slit to admit the cable, the slot being closed by a spring shutter *m* (similar to that described for the other apparatus) capable of being oscillated on pivots *n* in the ends of the box, and provided with wings *m′* to close the end slits in the box. A spring keeps the shutter closed until the cable comes on that part of it which covers the slot on to which it is directed by the prongs and the guide blocks *p*. The other side of the grapnel is similarly arranged, but the jaws and cutters are reversed, so that should the grapnel be turned round their action may be the same, and the grapnel may drag on either side indifferently.

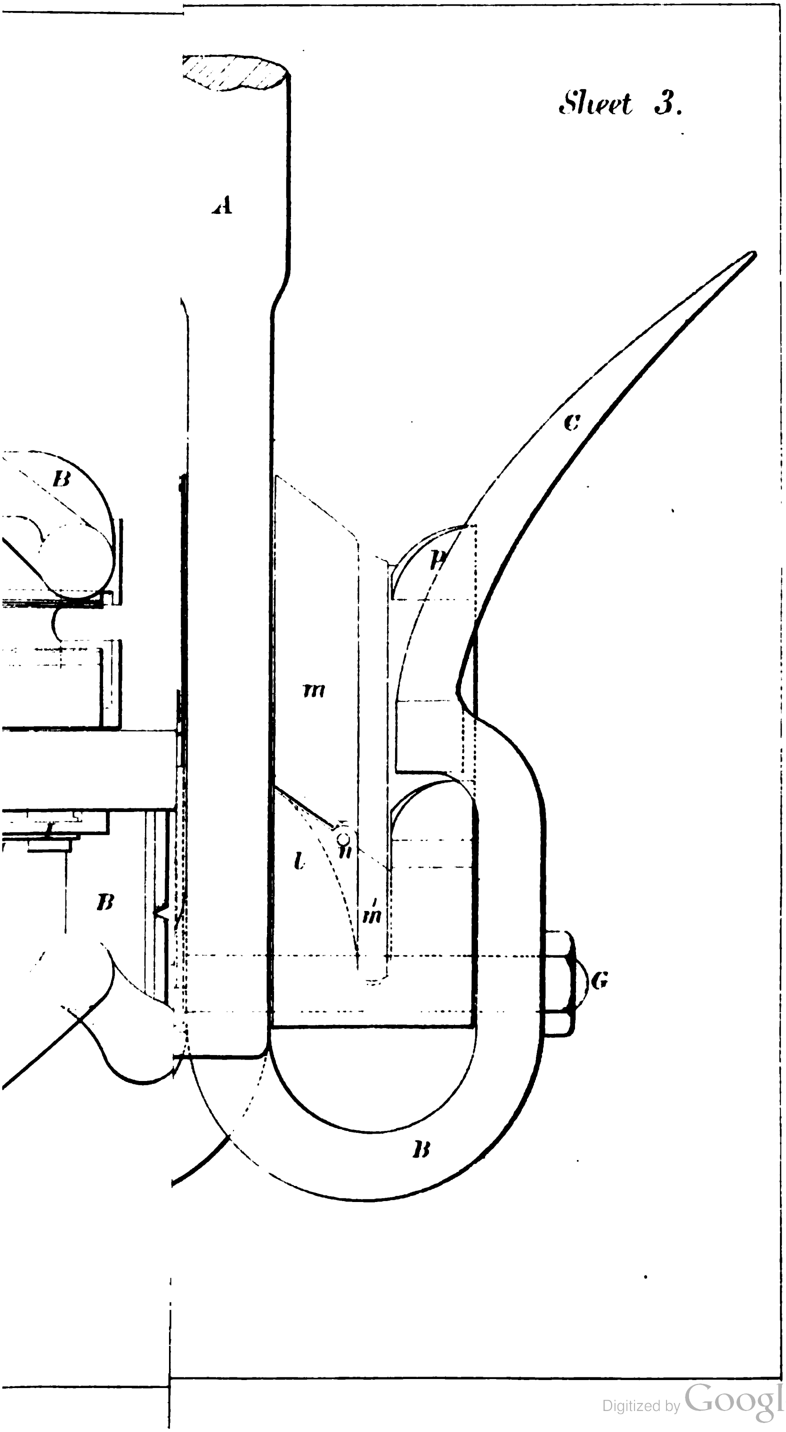
The CHAIRMAN: I am quite sure I only echo the opinion of every one present, when I express the acknowledgments of the meeting to Mr. Jamieson and Mr. Lambert, for the very instructive papers they have brought before us to-night; but before the pleasing duty devolves upon me of proposing a vote of thanks to those gentlemen, I suggest that we inaugurate a discussion on the subject; and probably from some whom I see present, and others of known experience, we may expect a very excellent discussion. Before that, I will call upon any gentlemen who have exhibited apparatus here to-night applicable to the purposes of the paper, to favour us with such a description as they may wish to give. We should like to hear what Mr. Schallehn has to say.

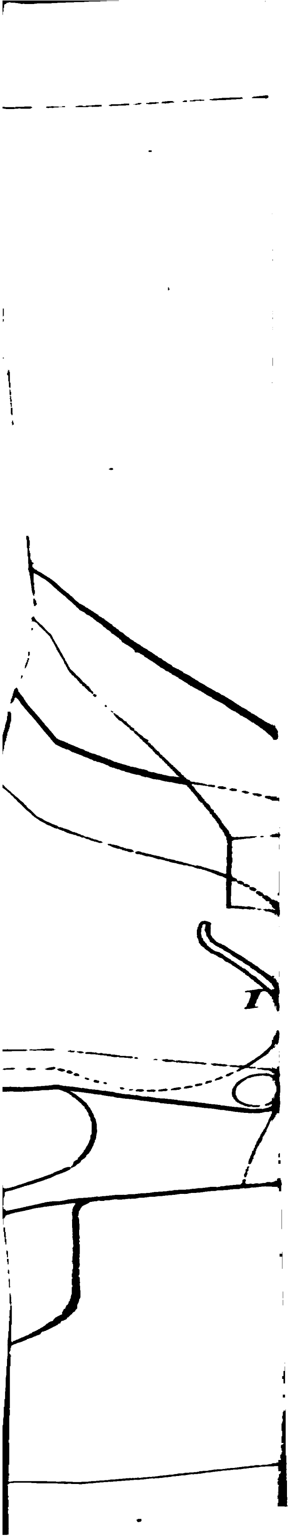
Mr. SCHALLEHN: I am sorry I am not able to describe the apparatus I have sent here sufficiently. I expected the gentleman who has attended to the matter for me would have been here, but he is not, and as I am afraid I should give but a poor explanation, I had rather be silent on the subject.

The CHAIRMAN: We have many present who have had great experience; amongst them, Sir Charles Bright; perhaps he will kindly commence the discussion.

Sir CHARLES BRIGHT: I have very little to remark upon the paper read, except to describe it as a very complete account of the operations which we are familiar with in grappling work. Mr. Jamieson has devoted a great part of his paper, very properly, to a description of his grappling arrangements. The form of grapnel is that with which we are most familiar—the 5-prong grapnel. The centipede I have not generally used, excepting for shallow waters and soft bottoms. The grapnel which Mr. Jamieson has described, and which, I believe, he is the inventor of, is a remarkably good one, as far as I can see from inspection, but I have not, however, tried it. It seems to me to fill up a gap in submarine telegraph operations. Several plans have been proposed for effecting the same purpose. I believe the idea is not a new one, for I think I have seen two or three patents for it during the time of the old Atlantic Cable, but I have never seen anything to my mind so practically applicable as the apparatus with the spring, which Mr. Jamieson has shewn to-night. I should like some time to be on

Sheet 3.





board to witness its operation, because it seems to me to be a very workman-like instrument, which I am sure will do good service in grappling. The form of grapnel introduced by Mr. Latimer Clark and Mr. Lambert, I have never seen at work, but I entertain some doubt myself whether you can combine cutting and holding at the same time with success. I think you get complication in the apparatus, and I am doubtful of the form adopted by Mr. Lambert being reliable for holding a cable. I doubt whether it would hold the cable, since the jerk produced on cutting might cause the cable to slip; also after cutting, since the cable is held so near its end, any jerk on raising would loosen the grip, and possibly lose the cable. I do not think I heard from Mr. Jamieson that he knew of any case in which the cutting principle had been used with practical success. There is no doubt the use of such a grapnel as that would save the loss of six or seven grapnels a day, as was experienced in the case of the Red Sea Cable, besides a good deal of rope. I should be glad to know what is the cost of this spring grapnel of Mr. Jamieson's, compared with the ordinary form; but I have no doubt it is much more costly, but its advantages over the ordinary grapnel, and the saving of time and loss of apparatus, may more than compensate for the extra cost of the instrument. At the same time, I think members would like to know about what the cost is. Looking at it from here, I should think it would run into a £50 note.

The history of grapnels which we have had is particularly interesting. I have no doubt there are gentlemen here who can give the history of other forms. I have tried a good many for holding the cable when cut, the history of which is almost forgotten by me. I have not picked up a cable since the remote date in telegraphy of 1873, although I began in 1853. I see here Mr. Frederick Webb, who could tell us something about grappling. As to getting up from the bottom, I should have liked to have heard a little more about the grappling rope than Mr. Jamieson has told us—whether he shackles at different points or at one point,—how he proposes to haul up the recovered cable,—in fact, more of the general arrangements of the tackle than the grapnel itself, to which he has more especially devoted himself. I quite

agree with him that no ship should be fitted for telegraph work without three bow sheaves. I was surprised to hear Mr. Jamieson propose—perhaps I misunderstood him—nipping on two sides of the grapnel and stopping a wire rope on to it. I think that is possibly an error: my own experience is different. I should prefer anything but iron to stopper-on the bight when I had got it to the bows. Mr. Jamieson spoke of the experience of Sir George Canning in using Manilla rope for grappling, and mentioned lightness as one of its advantages; but there is a disadvantage about it which has not been referred to. Its very lightness may occasion a great length of rope to be let out in grappling with it, especially with currents, as was the case in laying the cable from Portpatrick to Donaghadee, and also in the Gulf of Florida. I think the use of rope is disadvantageous in places where you want to get the grapnel to the bottom as soon as you can, and to get as small a curve as possible. I think the objection to the use of a rope purely of hemp is, that with a long train of rope out you are liable, if working on a rocky bottom with strong currents, to have the rope cut to pieces, and thus considerable time is lost, which would not be the case with a compound rope. Of dynamometers, I have seen several forms; but I think the old form which I used in the lifting of the old Atlantic cable seems to meet with general favour, and I believe it is universally used on cable ships, except in some particular cases. As to the form of picking-up machinery mentioned by Mr. Jamieson, time has not admitted of his giving more than a *viva voce* account of the plans used, but I hope it will be printed in the Journal. The most important thing, of course, is to have very powerful and well-made machinery. An engine with two cylinders is to my mind absolutely essential, though in old times they did without them. You want plenty of power, good machinery, and everything of the best kind. To have to go back 1,000 miles for new machinery is worse than spending a little extra money before sending out the ship. The tanks and other arrangements of the ship are not so important. The arrangements of the buoy ropes are important, and so are the stoppers from bow to stern. It would be tedious for me to offer any further observations. I see some of my old shipmates here, and I should like to

hear some remarks from them. In conclusion, I would say that the history of cable repairing operations which Mr. Jamieson has now given us is, I believe, the most collected contribution on the subject that we have yet had.

The CHAIRMAN: The names of Messrs. Johnson and Phillips have been mentioned, and perhaps one or other of them would like to make some remarks.

Mr. JOHNSON: I did not expect to be called on to give a description of the invention with which I am connected, particularly as in the present case it has proved, like all others for the same purpose, a failure. On the last occasion of my invention being tried, it was nearly successful, when Mr. Lambert raised the Atlantic Cable within 70 or 80 fathoms of the surface by its means. I am not sure whether any other patent of that character has achieved such success as that. It is not much to boast about. I suppose it has never been tried again; but I feel if I had another machine to design, I should design a better; but it is expensive, and takes time and money. I think Sir Charles Bright did not over-estimate the difficulties of satisfactorily combining the two operations in one instrument—one for holding and the other for cutting. It is easy to design a cutting grapnel, and one for holding, but to combine the two efficiently is, I think, difficult. Most of them have hitherto failed, because the cutting and holding have to be done at the same time. The thing is first to hold the cable, and then cut it, and that is a difficult problem. If you attempt to cut at the same time, as soon as that is done the strain comes upon the holding gear, and as it cannot be applied gradually, it generally slips through.

Mr. LOUDON (being called upon by the Chairman) said: I have no remark to make further than I have used Mr. Lambert's grapnel, and also that of Messrs. Johnson and Phillips, and I had greater success with the latter than with the former.

Captain STIFFE: I wish to say, in the first place, something about the bottom. On rocky bottoms two difficulties occur: one bearing upon the grapnel itself, referred to in the paper; the other is as to the tendency of the grapnel to hop over the bottom, and miss the cable. As far as Mr. Jamieson's grapnel goes, I appre-

hend it is successful in obviating the breaking of the grapnel, from holding on to rocks; but when it is released by the action of the springs, it takes a greater leap than the ordinary grapnel would do, and might miss catching the cable. Grappling on rocky bottoms might in many cases be avoided, by a deviation of route in laying the cable 10 or 20 miles; and if you can get a soft bottom it is preferable. If a cable is broken by chafing on a rocky bottom, if you repair it and relay it in the same place, it must go sooner or later in the same place, while such operations might be spared by the diversion of the cable from a rocky bottom to a soft one. I have myself relaid 60 miles of cable to avoid rock and get into mud, which was entirely successful, the cable never having broken since in that part. Reference has been made to the employment of pure Manilla hemp rope for grappling. I concur in the remarks of Sir Charles Bright on that; but there is one point in connection with it which has not been mentioned, that is, the liability of the grapnel, with a light rope of large dimensions, to fail to reach the bottom, and you find that the grapnel has not been on the bottom at all. With reference to picking-up gear, I saw yesterday a patent which seemed to me very promising.

[Captain Stiffe explained it by illustration on the board.]

Whether this plan has actually been tried upon cable work, I am not aware.

Mr. Bruce Warren: I am not able to add anything towards the engineering portion of this subject. There is one matter which I may refer to, although it is perhaps not one of very great interest to the telegraph engineer. The grapnel may materially assist us in determining the changes which have taken place and may still be going on with reference to the geological formation of coast lines, as well as the transitions due to geological time. Professor Agassiz has stated that the island of Marajo, which is situated at the mouth of the Amazon, formed, at no greatly remote period from the present, a portion of the main continent of America. In his belief the Amazonian formation formerly extended a hundred leagues out to sea beyond the present mouth of the Amazon river. There can be no doubt as to the rapid waste of land going on along the sea-shores of the mouth of the Amazon and of the coast eastward for a

long distance—a waste which is said to amount to as much as 200 yards in ten years in the Bay of Bragaza, or one mile in twenty years on the coast near Vigia. An interesting fact in confirmation of Professor Agassiz's deduction may be given. Some time ago I had to work on the repairing of a cable some distance from this coast, and for 200 miles from the mouth of the Amazon we frequently found the grapnel cutting through the soft sandstone rocks of the Amazonian formation. We sometimes fell in with extensive areas of clay deposits. It is not only to the geologist that such information may be interesting, for it becomes a question how far it is desirable to lay cables where the coast lines are undergoing such important changes. The absence of any marked accumulation of debris from the river points also to the fact of extensive currents seaward. I mention this as showing an interesting proof of Agassiz's theory with respect to the Amazonian formation of the Brazilian coast. An intimate knowledge of the geology of a coast line would in many cases enable us to tell what probably may be met with on the sea beds along the coasts. The effects of denudations in exposing more ancient rocks are alike of importance to the mariner and the telegraph engineer. The loss of a telegraph steamer in the Bay of Maranham should act as a warning in navigating along or near a coast where the indications of the geologist point to danger.

Mr. JAMIESON: In reply to Sir Charles Bright's remarks, I may say that he is not far wrong in putting the cost of the spring grapnel which I have brought before you this evening at about £50. You cannot make it complete, with gear, under £60, whereas the ordinary grapnel of 2½ cwts. costs about £15, so that, of course, this costs a good deal more; still, it has the advantage in its favour that it prevents a great deal of loss, and in one day's work will pay for its extra cost. With regard to grappling ropes, referred to by Sir Charles Bright, I have dealt with that very fully in my paper, both as to manilla and other descriptions of ropes; and it was only for want of time that I did not read that portion fully. I hope it will be printed, and that you will understand my views. With regard to the remarks as to the lightness of the manilla rope, it must be borne in mind that the cables are

getting older and weaker every day, and in many cases they will not stand grappling with a 6-inch rope; whereas in shallow waters —not speaking of rocky bottoms—if you begin grappling with a 6-inch rope, you feel very readily every indication of the action of the grapnel, and you are able to bring the ship more easily up, and take more care in grappling, whereas with a strong compound rope you walk right through the cable and never repair it at all. With regard to deep water, the letting out of an immense quantity of heavy rope is a disadvantage. I did not wish to convey the impression that I stoppered with a wire rope. Sir Charles Bright has filled up some gaps in my paper which time did not permit me to read. With regard to Captain Stiffe's remarks about the grapnel jumping, I may say that it does not jump more than the ordinary grapnel, because if you are working on rough ground you would not use these long toes, and you can put on weight to prevent the jumping. Practically, I do not think this grapnel jumps more than any other form, and if that is the only objection to it I do not think it will hold good. These are all the remarks I have to make.

The Chairman: In proposing a vote of thanks to Mr. Jamieson I am quite sure I may congratulate him upon the exceedingly clear and able way—indeed, the elementary way—in which he has brought before us the extremely interesting subject. In the commencement of his paper he alluded to facts which I should have expected would have led to a more extended discussion—facts which are interesting as to the relative durability of cables in deep and shallow water—the causes of deterioration to which cables are subject, such as those of abrasion upon rocks, &c., not excepting those due to that terrible little insect, the teredo. We have had many examples of its destructive effects upon cables, and we are anxious to know how far his instincts have driven him to invade the neighbourhood of deep sea cables. Mr. Jamieson alluded to volcanoes, rocks, and other causes of destruction; causes which I am sure ought to be brought before this Society by somebody who has had experience in the repairs of cables. One interesting fact he has alluded to, and which I have in my own short cable-repairing experience met with, is the nuisance and mischief that is occasioned by wrong charts of the ocean bottom.

A nation like England, with such a navy as she possesses, with plenty of half-pay officers anxious for something to do, ought to employ experienced men in surveying and correcting the charts, where great errors exist, so as to give the telegraph engineer reliable information to guide his operations. Mr. Jamieson alluded to an extremely ingenious apparatus for the guidance of telegraph engineers by night. That reminds me that not long ago, when I crossed the Atlantic, I saw off Sandy Hook an automatic buoy, which was very curious. It was an automatic pneumatic buoy about the size of ordinary buoys, in which air was compressed, which omitted a loud sound, hideous to hear, resembling the bellow of ten thousand buffaloes. It was situated about four miles from the shore, and being anxious to see its operation the captain of the ship obligingly went as near to it as he could safely, in order that I might watch its operation. It struck me that this peculiar description of buoy would be available as an additional aid to the telegraph engineer in his operations in foggy weather or at night. Mr. Jamieson has brought before us an extremely ingenious apparatus, and I am sure we are always glad of any little evidence of the fact that invention does still exist in England. We are all very much indebted to him, and, in proposing a vote of thanks, I am quite sure I record the opinion of every one present, and in that vote of thanks I am sure you will wish to include Mr. Lambert, for his interesting communication on the same subject.

The vote of thanks was unanimously passed, and the meeting adjourned.

The Seventieth Ordinary General Meeting of the Society was held November 27th, 1878, the President in the Chair.

After the transaction of the usual preliminary business, Major WEBBER, R.E., read a paper on

MULTIPLE AND OTHER TELEGRAPHS AT THE PARIS EXHIBITION.

Illustrated by ELISHA GRAY'S *Harmonic Telegraph.*

There have been epochs in the history of each branch of applied science, which, to those interested in their progress, mark fresh points of departure in the struggle for improvement. In the case of Telegraphic and Electrical Engineering, the various International Exhibitions have happened to coincide with certain stages of actual improvement and discovery, and there has been hardly one at which some fresh invention or application has not been almost for the first time made public; and this has been no less the case this year at Paris than in the past.

These coincidences are, of course, only accidental; but we have no less than others a duty to feel grateful to Exhibitions for the part they play in making public that which, but for them, might lie hidden for years.

Unfortunately, at Paris the various apparatus which, if brought together, would have formed a very interesting collection, were scattered up and down the building, and only in the French section was any display made convenient to the professional and general public.

As the only foreign juror for Class 65, I have, therefore, ventured to give to our Society a trifling notice of what could be found, believing that our Journal ought not to be left without any information as to one of the most important collections of telegraphic apparatus which has ever been brought together.

It may, also, be interesting to place on record the names, etc., of the Jury and its assistants, as all are members of this Society :—

Jurors.	Jurors Suppliante.
Mons. Eduard Becquerel.	Le Capitaine Huc.
Mons. Bergon, of the French Administration.	Mons. Baron.
Major Webber, R.E.	C. E. Spagnoletti, Esq.

Expert to advise—Mons. le Comte du Moncel.
Acting-Secretary—Mons. Clerac.

It is of no small importance to the telegraph engineer, and to those who are interested in telegraphy, that, at all International Exhibitions since 1851, this application of electricity as an agent has been classed separately, and thus placed upon an equal footing in this respect with railway engineering; showing the estimation in which it is justly held, and the future which it is expected to attain.

In Class 65 at the Paris International Exhibition, so-called telegraphic apparatus have only been included, confining the class to the application of electricity as a means of communication, and only departing from the rule to make it embrace apparatus in which pneumatic means of motion are employed.

The consequences are, that the engineer or scientist who deals with all applications of the use of electricity as an agent, has to search in other Classes for the electric light, the electric pen, and other well-known apparatus.

Perhaps this system of classification may be deemed to suit best the popular understanding, but the real reason may appear plain, by the fact that the National Commissioners found that the easiest way to arrange the French section, and promote its chances of success, was to invite the French Telegraph Administration to take charge of its organisation, and bring together all the objects which are made in France for telegraphic purposes, and of which the Administration is naturally the largest customer.

While ensuring a very excellent exhibition of French telegraphic apparatus, this system of classification had the effect of marring the display for the other countries, which in this particu-

lar class each sent, as a rule, but two or three exhibitors. If the apparatus for all the practical uses of electricity had been in the same class, it is probable that other countries would have brought into proximity with one another, very interesting groups of objects, allied to one another by the use of a common motive power.

In some cases the rule *was* departed from: thus, in the Austrian section, each of the State Railway Companies showed very interesting and well-made samples of their system of signalling, which, though not suited to the rapid and frequent passage of trains with us, are fully equal to the safety of the traffic in that country.

Any one interested in Class 65 would have found difficulty in finding the objects exhibited, as well as in believing that there were in all about 180 exhibitors, except in the French section, where all were collected into one fine annexe close to the Porte Rapp; and the meeting will be pleased to hear that 139 awards were given in the class.

In the front rank, the French Administration showed a most interesting collection of the apparatus in use for the national telegraphic purposes, and, by placing the charge and arrrangement of it in the able hands of Mons. Clerac (one of the members of this Society), ensured the most useful results.

While exhibiting as an Administration, the Public Department was not forgetful of the inventive genius of its employés.

In the French catalogue are to be found a list of some twenty names of gentlemen in the various ranks of the service to whom are due either the invention or application of improvements in apparatus which have been more or less adopted, or which, by their ingenuity, show the intelligence and the talent of the public servant, whose patient labour and zeal for the service in which he is employed are thus once gracefully acknowledged to the public by the generous-minded men who have found their way to the head of the French National Telegraph Administration.

The most remarkable of the class of objects exhibited at Paris in 1878 are, the accoustic telegraphs (including the telephone) from America, immortalising the names of Elisha Gray, Bell, and Edison, and the multiple telegraphs of Meyer and others, improved by Baudot, of Paris, and Otto Schaeffler, of Vienna.

In 1867, France took the opportunity of making the adoption by so many countries of Hughes's type printer the occasion for according him the "Grand Prix." On that occasion, Professor Wheatstone was the juror for the United Kingdom, and his beautiful inventions, then in advance of all the world, were *hors concours*. In the interval, during which those inventions have done such good service to England, our lamented friend has left us, and his apparatus, unsurpassed in beauty and efficiency, will never receive the public award which they are entitled to. Had the Professor been alive, there can be no doubt that but one place, namely, the head of the list, would have been his; for in no country are his titles to honour better recognised than in France.

To any one who has studied the objects exhibited in Class 65 by Continental nations, it will become very evident that invention has striven chiefly towards modifications in type printing apparatus. This is easily accounted for by the importance attached to two conditions: one, the having an unmistakable record, which need not be copied; the other, the being able to dispense with the use of the Morse alphabet. It is a very remarkable phenomenon in telegraphy that these results should be made of primary importance in France, and rejected, as of insignificant value, in England;—that, while in the latter country type printers receive no encouragement, and are completely abandoned in the State telegraphs, in the former their improvement has been the object of so many inventors, both inside and outside the Telegraph Administration.

This fact may be partly due to national character, but it will not be evident to the public visiting the Exhibition, because the English and German State telegraphs have abstained from contributing—an absence much to be deplored by the telegraphic world, and in the minds of some even amounting to a neglect of public duty, considering the large monopolies in the application of one branch of applied science enjoyed by these Departments of the States named.

We all know that the Post Office is not responsible for the acquisition of this monopoly; and it is a subject of much congratulation, both to the Post Office and the telegraphic world, that the Society of Telegraph Engineers sprang into existence the same

year in which it commenced in the United Kingdom, tending to counteract the evil effects on scientific research which want of competition inevitably gives rise to.

The Paris Exhibition of 1878 has happened in a momentous year in the history of telegraphy, and there can be no doubt that what has there been shown will be the basis of a fresh start, in spite of the unfortunate absence of two of the most extensive employers of electric force.

It is to be hoped that in the year 1879, if the Telegraphic Conference takes place in London, the Society of Telegraph Engineers will make a grand effort to obtain co-operation, not only at home but from abroad, so that, if possible, the largest and most modern and complete exhibition of apparatus may be brought together that can be collected. We know what *our* country can show, and Paris this year enables us to judge what other countries could lend us, if sufficient inducement could be given.

Of type-printers only one, namely Wheatstone's, came from England; the communicator used with it is the same as that in his well known Alphabetical Telegraph, now the property of H.M.'s Postmaster-General.

This, with many other beautiful instruments, was exhibited by the British Telegraph Company, whose representative in Paris was our Honorary Secretary, Mr. John Aylmer.

Mons. Tiral, of the French Administration, shows how a Hughes can be worked duplex with one key-board and two printing wheels, and with Mons. Mandroux (also of the Administration) has effected a saving of the resistance of the local magnets some 12 to 15 hundred ohms, by a mechanical arrangement which cuts them out when sending, while the "*local*" prints mechanically by the work of a spring on the inducer; this, with a compensator of the make and break currents in the bobbin, has immensely facilitated the use of Hughes' type printer for duplex working, and both arrangements have been already extensively adopted.

Mons. Girabon, belonging to the French Administration, has made and exhibited a key apparatus to be worked automatically, and which can be attached to any Hughes instrument.

Amongst several additions to the type-printer, displaying great

ingenuity, I should not like to pass over his "composing chain," which can be used in place of the punched paper slip in his Hughes or Morse automatic transmitters (Fig. 1, plate 1).

The chain is formed of metal cross bars (*a a a*), movable between two double rows of longtitudinal bands (*b b b*), which are held by flattened rings (*c c c*) in their places—one ring between each cross piece. These latter, which can be pushed from side to side between the bands, are prevented being pushed too far by a little boss (*d d d*) on the face of each.

Each cross piece is marked with a letter of the alphabet, and with a number or sign, and the chain may be as long as is convenient; consisting of series after series of alphabets, with one blank for the letters and another for the figures, exactly as in the key-board and type-wheel of the Hughes apparatus.

To compose a message, it is only necessary to push out on one side the successive cross pieces, marked with the letters forming the words, and to push out on the other side all the cross pieces between them.

The composing chain for Morse signals can be made much shorter, although constructed on the same system.

The cross pieces and the flat rings are much narrower; they are not lettered, but one pushed out represents a dot, two a dash, and one left unmoved represents a space between signs, and two a space between letters.

This composing chain, in travelling through the transmitter, passes round on a drum, which revolves with the type-wheel; each cross piece finds its bed in an indentation on the drum lettered to correspond, and at one point of the revolution the projecting end of the cross piece which has been moved to that side, acts on the rotating sledge of the Hughes, or as the hand on the key of a Morse, and by a similar movement excites the recording armature.

I have brought this before you more on account of the originality of the idea than from any experience in its application. The time taken in composing and passing the chain through the mill would materially affect its value, at the same time the skill of the labour in composing would be far less than that required for punching a Wheatstone slip, and the work would be well adapted

for female hands, while the wear and tear on the machinery on account of the length of the chain would be increased.

The length of the Type chain for a message of twenty words is 8 feet, and of the Morse about 6½ feet.

Mons. Ailhaud, one of the heads of the Administration, has applied his talents to the same subject in another direction, and developed an arrangement for working Hughes' printer's duplex without the use of condensers, an important achievement, if successful, for the French Administration, which at present has only obtained the right to use those of Stearn on the Paris, Havre, and Paris Brussels lines, and for this a Royalty is paid.

Mons. Ailhaud's method for compensating for the charge and discharge, is to attach a commutator composed of sectors of a circle, at the end of the axle which carries the cams. On the face of this moves a contact rubber, which is turned by the aforesaid axle, and which, in passing over two sectors at the moment that the transmitting spring makes contact, derives a portion of the line current to earth through a resistance, thus as it were lessening the charge, and then, passing immediately afterwards over the sectors following, introduces the current from the line battery at the station into the artificial line, and thus at that moment neutralises the arriving current of the distant station, and at the same time neutralises the discharge. This system is described in detail in *Annales Télégraphiques* of January, 1878.

Good specimens of the type printers of Dujardin and Chambrier were also shown.

Meyer.

The multiple apparatus of Mons. Meyer, of Paris, is too well known to require any detailed description; but some information given me by the inventor himself will not be without interest.

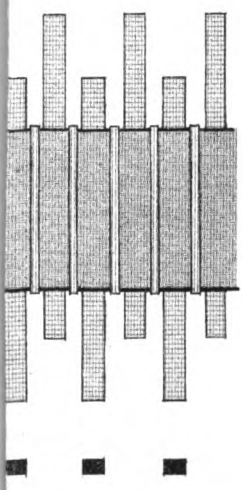
His system in quadruplex first assumed a practical form in 1871.

In 1873, Mons. Meyer received at the Vienna Exhibition a "grand diploma." Since that year he has devoted much time at his works, No. 22, Boulevard d'Enfer, to bringing the apparatus to the state of perfection shown at the Exhibition this year.

Mons. Meyer also informed me that his system has worked

Plate 1.

Fig 1.



440.

between Paris and Lyons, and Lyons and Marseilles, between Amsterdam and Rotterdam, between Berne and Zurich, Rome and Florence, Naples and Messina, between Vienna and Prague, and Trieste, also between Berlin and Frankfort; actually carrying, as given by official records, an average of 100 to 120 messages an hour as a quadruplex, and 130 to 150 as a sextuplex.

MANDROUX

Exhibited a continuous current duplex, which (see Fig. 2, plate 2) requires—

First. A relay x is wound with two wires in the same sense, joined as described below, the one to line, the other to act locally and neutralise the outgoing current in the first.

Second. An electro-magnet y wound as follows:—One of the bobbins, on the right at station A (left station B) is wound with the line wire from x whence it goes to line. The other of the bobbins is wound with a wire which is in circuit with the second wire on x. This local circuit thus traverses both bobbins of x, the left hand bobbin of y, the bridge of the key, a rheostat R, and at each end goes to earth.

When the key is depressed the battery current divides at o, half goes to line, the other half divides again at the bridge of the key, half going through x and left hand bobbin of y to earth, the other half through R to earth.

When the stations are joined up to work with opposing currents, the key not being depressed, these currents neutralise one another in the line. When the key at A is depressed, A's current goes to earth by the two routes mentioned in the last sentence, the rheostat regulating how much divides at the bridge. B's current then preponderates and moves B's armature, but not that at A, because B's current in A's relay x is neutralised by the amount of A's current, which the regulation of R sends in a contrary direction through the second wire of x. Thus A regulates his balance by means of R.

When the keys are depressed at both stations at once, the greater portion of the currents pass into the local secondary circuit; the balance is upset, and the armatures work under their influence.

In fact, when both keys are depressed both armatures are closed, but the raising of A's key relieves B's armature, and *vice versa*.

When the stations are joined up to work with the same current, the key not being depressed, the sum of the currents at the two stations passes to earth, and the armatures are attracted.

When A's key is depressed, his current nearly all passes through the local, which relieves the armature at B, but that at A continues to be attracted because of the local current which enters x in the same sense as that from line, and maintains the strength of the relay at A.

It is probable that the plan of working with opposing currents would prove the best.

In each case the electro-magnets y y' are arranged so as to compensate for discharge. At the moment of charging the line an induced current passes in the opposite sense through the secondary circuit of the relay x and the left hand bobbin of y (right of y'), neutralising the effect on the relays. The intensity of the induced current could be regulated by a rheostat placed in a shunt at R^2.

Olsen.

The telegraph instrument of Mr. Olsen, of Christiania, deserves notice as an improvement on the model described by him in the *Journal de Berne* of 1876.

As readers of that paper are aware, the apparatus consists of two parts:—

1. The perforator;
2. A transmitting and receiving instrument;

The last is capable of use either as a manipulator or automatically, the greatest speed being of course obtained when used in the latter way.

In the perforator the improvements consist of a new arrangement of cams, which act on the perforating axle and its three punchers.

In the transmitting apparatus, the improvement consists in being able to send seven characters instead of five during one revolution of the type wheel. When recourse is had to automatic

transmission, the receiving portion can travel easily with a rate of 180 revolutions a minute.

The arrangements of the type and correcting wheel, as well as changing to figures, are precisely the same as in the Hughes.

For the means of correction, Mr. Olsen claims an improvement, as his arrangement gives a duration nearly double, viz., as 56 is to 30. On the other hand, he is obliged to employ two separate sets of clockwork, which only work as one during the revolutions of the printing shaft—one working only with the printing shaft, the second running constantly; however, the action of the same treadle moves both.

The work done by the perforators in the paper slip is identical with that in Wheatstone's and so many of his followers, but in this apparatus the maximum result of alternating currents is more attainable, much limited in Wheatstone by their varying length necessary to produce the dot and dash of the Morse Alphabet.

The apparatus is arranged to be worked duplex, and for this purpose Mr. Olsen has constructed a rheostat and condenser in one, of which, however, I have no description.

Baudot.

The Baudot apparatus exhibited in the French section at Paris is constructed so that there are five communications through one wire; that by Schaeffler in the Austrian has four.

The various apparatus are fixed on one large strong counter, and are so arranged that the key-board for each clerk is as close as possible to the type-recording receiver, which corresponds to the line of communication. (See Fig. 3, Plate 2).

Each key-board (or manipulator) consists of five keys; each key when at rest is connected with the negative pole, and when depressed, with the positive pole of the line battery, the centre of which is to earth. The manipulation of these five keys allows of 31 combinations.

At each station there is a distributer (Fig. 4), in some respects on the same principle as that in Meyer's instrument, the description of which is to be found at page 862 of the "Electric Telegraph," by Prescott. The body of this consists of an ebonite cylinder,

arranged, with five rings of brass contact-makers encrusted on and let into the ebonite; all the wires from the five key-boards lead into the end of the drum, whence also proceed the various wires to other parts and to line. The circumference is divided into six sectors, one small one used for correcting synchronism, and five large ones used for sending and receiving; each of these sectors is divided (see Fig. 3, which shows the surface of one sector in plan)—

A first, row of five contacts, l^1 l^2, etc., for the local work.

A second, row of five contacts, E^1 E^2, etc., for receiving.

A third, row of five contacts, L^1 L^2, etc., for sending.

A fourth, one single contact through all the sectors, completes the sending circuit.

A fifth, one single contact through all the sectors, completes the receiving circuit.

These last two are switched into circuit by the operator for sending and receiving.

In Schaeffler's apparatus there are four circuits, and in his key-board the five keys are worked on with an alphabetical key-board like in an ordinary Hughes, saving the clerks having to carry the combinations in their memory.

The 25 contacts of the first row thus correspond with the five key-boards, and through them the active currents from each key pass to line.

The 25 contacts of the second row are connected with the "combiner" at the station.

Those of the third row are used to work the local type-printing discs, and record the signals sent.

The revolving limb A, which carries the contact rubbers (see Figs. 3 and 4), is attached to a hollow axle, borne on the main axis of the instrument, and is moved by powerful clockwork which causes it to travel round the drum, the path of each of the contact rubbers being on the face of the ring which it serves.

The rubbers are connected electrically in pairs. See Fig. 3.

The first connects row marked l with E.

The second, row L with the sending row.

The third, row E with the receiving.

At each station the clockwork above referred to, carries the limb of the distributer slightly in advance of its time, and as it passes underneath, this error is corrected by the action of an electro-magnet, which receives its impulse from the current carried by that sector of the drum already referred to: thus a perfect synchronism of the revolving limbs of the distributers at each station, at one position in each revolution, is established.

Without more illustration a reader cannot possibly identify all the parts of this portion of the apparatus, but he will understand that its object is to secure that at the same instant of time the contact rubbers at each station shall arrive at the corresponding contact points of each series.

Each of the operators at the sending station presses the keys necessary to send the series of currents required to produce the combination (described further on) at the receiving station. As the limb passes over the series of contacts on the distributer drum which belongs to the key-board worked by each clerk, the combination of each is sent to line, and regularly in succession received on the distant distributer; if no keys are pressed, nothing passes while the corresponding series is under the limb; if the keys are pressed at irregular intervals, no irregularity in transmission occurs, because the transmission only takes place during that one position of the limb.

The reader who does not care to follow the details, will understand that this limb which is whirring round the drum, is sending currents to line in combinations of five, which are travelling to the distant station as fast as they can be received at the other end, without merging into a solid current.

It is already understood that these are received at the distant station on a similar distributer, which, when acting as a receiver, passes the currents in precisely the order they have been received by the corresponding contacts to five groups of five small polarised electro-magnets (1, 2, &c., Fig. 3). The arms of these relays, it is needless to say, maintain the contact made by the last current which has been received by the distributer, and each group of five, after each revolution of the receiving contact rubber of the limb, remains *in statu quo* until the sending station sends another set

of currents to group the arms of the corresponding five relays in a fresh combination.

It will be recollected that the keys at the sending station, when at rest, cause a negative current to pass to line, therefore it will follow that, until disturbed, the five relay arms will all lie the same way; negative currents will not therefore move them, except they have been left on the positive contact in the transmission of the previous combination.

The receiving apparatus special to each of the five lines of communication consists, as has been said, of a set of five commutating electro-magnets and a combiner (Fig. 5), with a type-printer attached.

The combiner is constructed as follows:—A fixed ebonite drum carries five rows of brass encrusted contacts; round this travels a limb B (Fig. 5) which is on the same axle as the type wheel, bearing five contact rubbers, the contacts of which are in the same straight line parallel to the axis of the drum. In Fig. 3 the face of the drum is shown in plan.

The contact makers occupy three quarters of the face of the drum, the remaining quarter presenting a surface of plain ebonite; in each ring the contacts are insulated, but alternatively connected with the plus and minus contacts of the relays, *a a′ b b′*, &c., each relay serving one ring.

Thus, in the first ring which has 16 contacts, eight lead to the plus contact, and eight lead to the minus contact of No. 1 electro-magnet.

In the second ring which has 9 contacts, five lead to the plus, and four to the minus contacts of No. 2, and so on.

It will now be easily understood that, as the limb with the five rubbers revolves round the drum, there will be only one position in which it will complete the local circuit through the contacts on the drum and the relays, for each combination of the latter, and how, at that moment, the printing lever C will strike up against the type, and record the character.

As a negative current is always passing to line, the transmission of each character requires at most three changes of current from minus to plus:

Thus C is transmitted by + — + — +

Several letters only require one.

Thus A is transmitted by + — — — —

E is transmitted by — + — — —

so that each revolution of the distributer sends at most 15 currents and at least five.

When there are two or more currents of the same kind following one another, the impulse of the second and following currents is diminished, as in the Wheatstone transmitter, by an arrangement of the key, which reduces the strength.

Naturally, the rate of revolution of the distributer limb depends on the rate at which distinct impulses of current can be received at the distant station.

In aetual practice, each impulse can be easily distinguished when it occupies only $\frac{1}{75}$th of a second on a line of 630 kilometres in length; but as the limb travels at a uniform speed, its passage over each sector of five contacts must occupy about $\frac{1}{15}$th of a second, which in practice allows of the limb making from 120 to 140 revolutions in a minute, and producing from 10 to 12 characters a second.

Owing more to the continual objections which he met with to his electrical combiner, and also in order to introduce several important modifications, besides reducing the number of points of electrical contact, Mons. Baudot constructed an instrument which he calls his No. 2 Form.

The alterations are very extensive, and show the genius of this very modest, clear-headed, and practical man, who is a bright example to telegraph operators in every country, he having commenced his study of Electrical Engineering a few years ago in the spare hours from his daily duty, and, having proved to the French Administration the value of his investigations, has had his time placed at his own disposal to perfect them, and labour and funds allowed to him.

No. 2 has at present only been constructed for a station with two circuits on the same wire, and this Mons. Baudot calls his unit.

The counter is a strong table, about 4′×3′, the key boards are at opposite sides, near the opposite ends, and the apparatus, con-

sisting of distributer, combiner, and the clockwork, is placed in the middle. (Fig. 6, plate 3.)

The first stage in simplification is in the distributer. Instead of a cylinder we have a flat ebonite disc A. Round its edge on the face are encrusted platinum contacts. Laid on this disc with coincident centre is a brass star (one ray lettered B), having the same number of rays as there are contacts. The axis of this is connected with line, each ray has a platinum contact on its under side, and they all turn upwards, so that pressure is required to make them touch the encrusted contacts underneath.

Contact is made by a limb C, which having the same axis, travels round, pressing down the rays by means of a little ivory wheel; the limb strongly resembles the grinding wheel of a mortar mill. At each station the little mills must travel synchronously within certain limits, to be described later on.

In the No. 2 we are considering there are 14 contacts, namely, five for each circuit, the remainder for the correcting current and to space between the series.

This is a single distributer. It is only required for *receiving* the currents from the keys and sending them to line. Owing to the use, in form No. 2, of only one electro-magnet in the receiver, a special distributing disc for *it* is unnecessary, and a distributer for recording locally has not been used, although it could be added.

The combiner (Fig. 7) for a pair of circuits, as just said, requires only one electro-magnet B B; this, influenced by the successive currents received through the distributer, causes an axle, A, which carries two upward turned hatchets, C, to give the latter a rocking movement.

As these currents arrive in two series, in each unit two hatchets are required. At this point the mechanical functions of the new combiner replace the electrical ones in the No. 1.

The arrangement is in duplicate, one over each hatchet. (Only one is shown in section in Fig. 7,)

Five small steel forks, D, stand vertically over the hatchet. Five cams, E, revolving by the local clock-work carrying projections at *x*, cause the forks to descend, first in succession, and then altogether.

once in each revolution, after which they rise by the force of a resisting spring attached to each.

The direction of the descent of each fork is guided by the position of the hatchet. Each has a projection (y) on one side, which only acts when a positive current places the hatchet so that the fork is pushed out of the perpendicular on the side of the projection, which then, in its descent, catches and depresses a corresponding projection, which appears out of a hole in the curved side of a small cylindrical (pin) box Z. This latter projection W is part of a small thin steel disc, toothed on the edge, of which there are five in the box, lying side by side, one for each fork. The toothings are so arranged that when the discs are laid one against the other, so that the projections coincide, there is *no* line found on the circumference of the cylinder thus formed, at which the toothings present a clear cut in the face, but when any one, or combination, of the projections is moved, the toothings will at once present a clear cut.

In the cylindrical (pin) box, and round its circumference, stand 31 small steel pins, F, which project through slots on one flat face of the box, and which, by means of springs, press against the toothed edges of the discs (springs and projecting ends not shown).

At each combination of the toothings on the discs, a pin falls forward into the cut, and its projecting point comes in the way of a little limb which travels round on the face of the box within the circle formed by the projecting points of the small steel rods, and moved by the clockwork, somewhat similar to the sledge on the rotating axis in the Hughes type printer (not shown). The impediment causes the limb to cant, and acting by means of an arm lever on a catch, frees a spring which brings the printing lever in contact with the type wheel ; the printing lever immediately after resumes its normal position in the catch, pushed thereto by a cam on the printing wheel.

A further modification of the distributer, and a complete change in the combiner, has been made by Mons. Baudot in his Form No. 3, which may be considered his future working model.

The arrangement for having one counter for two operators is adopted, the distributer is separated as in No. 2, and may be

placed by itself on a table apart; it is increased in capacity, and can distribute to and from 3 such counters or 6 operators at each terminus of a wire; being arranged so that a conductor may be worked singly or sextuplex. The distributers then have their own clock-work. The distributer which in No. 3 is of the same altered form as that described for No. 2, has $5 \times 6 + 12 + 2 = 44$ contacts, 5 for each of the 6 operators, 2 between each series of 5, and 2 for the correcting current. The mill wheel limb travels round as described, pressing down the 44 rays of the contact star.

Three of these distributers are used—one for sending, one for receiving, and one for the local recorder, when that is used.

Synchronism of movement at the two stations is, as has already been explained, obtained by the passage of a correcting current, which, acting on a local, delays the travelling limb, which may be in advance of the other at the end of each revolution, but a further improvement is effected in No. 3, which avoids all chance of irregularity of movement between considerable limits, such as might cut off too large a portion or the whole of a signal. It is found that contact during only $\frac{1}{5}$th or $\frac{1}{8}$th of the time of passage of the mill wheel limb is necessary to transmit the line current, and, therefore, the limb carries on its end a small ratchet discharger, which derives the line current to earth except during a fraction of the passage of the wheel, and by which that fraction of current to the combiner can be made to take place at any instant during the passage.

It is obvious then that all trouble as to inaccuracy in the synchronous movement of the travelling limb at each station is quite avoided.

Six series of 5 wires are led from each distributer to the three operating counters. Each operating counter has its own clock-work, its combiner, with two printing wheels and two key-boards. Again, in No. 3 as well as in No. 2, we find a mechanical arrangement replacing the electrical one in No. 1.

The one electro-magnet and two combiners in No. 2 are replaced by five electro-magnets and one combiner.

The five electro-magnets are wound for intensity, and are connected to two contacts on the receiving distributor—No. 1 magnet

to No. 1 contact first series and No. 1 contact third series, and so on.

Contact completed at the distributer, the line currents pass through the coils to earth, but, at the moment before the mill wheel releases the contact ray, a local circuit through the electro-magnet just affected by the line current is closed. The work of these two currents on the electro magnet is as follows:—Each electro-magnet has a keeper, with a spring to release it. The local current powerfully attracts the keeper, and there is enough residuum to continue the attraction. Each negative current leaves the keeper attracted, but each positive releases the keeper, which strikes upwards in the act, to be re-attracted by the immediately following admission of the local battery.

In place of two combiner boxes there is one disc combiner, constructed as follows (Fig. 8):—

One face of a brass disc, which is about 3 inches in diameter, is ploughed into 10 concentric grooves, which are divided into deep or shallow sections, the number and length of which in each groove agrees with the rule by which the combinations in each of the three systems are obtained. At one line of radius H I of these concentric circles the divisions between five alternate grooves are obliterated for about $\frac{1}{8}$th inch, and at the same places five little studs K project through slot holes into the grooves from the back of the disc; each little stud belongs to one of five levers, which when moved at the further end moves the stud in the slot. The play of these studs when moved is just sufficient to change or shunt the direction, from right to left, and *vice versa*, of any object travelling round in the groves.

There are five such points or fingers L which press into the bottom of the grooves, on each of two limbs M N which travel round the face of the disc, one limb for each circuit, and these fingers each have two grooves, into either of which they are shunted as described.

The shunting levers are connected by a beautiful and simple reciprocal movement with five little rods; each rod is end-on to the keepers of the five electro magnets. These keepers when released, as already described, by the incoming positive currents, spring against and strike the rods, which guide the five little shunts, which

thus assume a fresh combination of positions as each set of currents arrive, and are ready to guide the fingers into the corresponding grooves on the disc. In due course each limb which carries the fingers, and which is moved by the local clockwork in its revolution, reaches the radial line at which the ten grooves merge into five, acted on or not by the shunting studs underneath, each finger continues in the same groove or enters the next one, and then travels on with its fresh combination until the radial line is reached at which no section of shallow grooves is encountered by the fingers. As they act as one in this direction, they all fall into the deep grooves for a moment, and the limb in changing position acts on a catch which frees a spring, which, as in No. 2, brings the printing lever in contact with the type.

In comparing his with a Wheatstone automatic apparatus worked duplex, Baudot claims the following superiority :—

1st. He saves the time given to perforating and writing down the message.

2nd. He also saves the labour, two operators sufficing for each line of communication.

3rd. He saves the errors in punching and writing out.

He claims equality electrically, as both systems send continuous currents, and both reduce the current for continuous emission of the same kind.

He admits inferiority on the subject of the synchronism, but claims that his precautions preclude almost all possibility of error.

In his No. 3, his arrangement of counters is considered very convenient, the counter carrying the distributor might be in charge of one operator to watch it, but in fair states of lines, could be left alone for hours, the separate clockwork for the distributer and for each operating table is also convenient.

In No. 3, he arrives at 150 to 180 revolutions of the limb a minute, this gives 900 to 1,280 characters, which, at 150 characters to the message, produces the enormous result of 360 to 500 messages an hour.

There is no pretence on Mons. Baudot's part that such work has yet been done, because his No. 3 form is only still in course of construction, at the works of Dumoulin Froment, at Paris, but from

the practice over a long time on working lines of considerable length, which he has had with his first forms, there seems to be little reason to doubt a very astonishing measure of success.

SCHAEFFLER.

It would be impossible to describe in detail the wonderful multiple telegraph exhibited by Mons. Otto Schaeffler, within the time I have at my disposal, but I can place the meeting *au courant* of its principal features in a few words.

The apparatus itself standing on one massive table with strong cast iron supports, is a marvel of design, in which the proportion of strength of each part bears evidence of careful thought.

As already stated, each of the four manipulators use an ordinary alphabetical key-board, which by mechanical transmission acts on the five keys, which Baudot's manipulates with the fingers.

These keys at rest no current goes to line, but when depressed, altogether or in groups, currents are sent to a distributer, the office of which, as in the Baudot, is to send them successively to line. This distributer is (Fig. 1, plate 4) in the shape of a disc of ebonite, on which revolves on an axle, having the same centre, a limb Z provided with three rubbers, the two inside of which are connected electrically with one another, and not with the outer one.

On the disc are encrusted three rows of contacts. In the outer there are five line contacts in each quadrant, besides some others; in the middle one, also, five in each quadrant, each connected to what Schaeffler calls his permutation relays, R_1, R_2, &c. The inner is composed of a metal ring which is connected with one pole of the local battery, the other pole being to a relay which closes the local with each line current.

The object, then, of the line currents passing through a distributer and a local relay at each station, and then to earth, is, to move the armatures of the five permutation relays of each communication. The armatures of the relays (which in Baudot are polarised) are restored to their normal position when the current has done its work, by a mechanical action somewhat

similar to the one which is used in the Hughes instrument for the same purpose.

It is essential to describe how the combinations of positions of these five local relays are made to act on the printing relay belonging to each of the type wheels of the four communications, after which there is nc difference between the rest of Mr. Schaeffler's arrangements and those in the ordinary Hughes apparatus.

Of the 31 possible permutations, 28 are utilised to correspond with the 28 signals on a Hughes keyboard and engraved on a type-wheel.

The principle of the "permutation relay" may be understood as follows:—Figs. 2, 3, and 4, show three positions of a group of levers, I, P, and W, of which the axes are at the centre, again shown in Fig. 5, with their surroundings.

The screws m and m' govern the movements of lever I.

Fig. 3 shows the upper end of lever I pushed to the right, pushing P with it, and W, through the screw v', which depresses the fork G, so that the catch of the spring h enters the higher of two notches in the ratchet k, between the jaws of the fork. A glance will show that the raising and lowering of the fork G is governed by the position of the levers.

In Fig. 5, lever I is shown carrying an electro-magnet, $x x$, having a soft iron core, which is placed in a rectangular frame which forms part of lever I. This is the permutation relay.

This lever, complete, Mr. Schœffler calls his "impulsor." The permanent magnet M is screwed on the plate B, the lower arm is longer than the upper, and to it is attached perpendicularly a polar plate N, about 3 centimetres long.

It will be seen that the impulsor can oscillate on its axis under the influence of the poles of the magnet, guided by the current passing through the coil, and regulated by the screws m and m'. The name "impulsor" now explains itself, its oscillations move the levers I and W, under the tension of a spring f attached to an acute angled lever d, of which the fulcrum is fixed to that part of the frame marked g, which turns to the left between the arms of the magnet; the tension of this spring f is regulated by the screw v^2, the head of which appears in Fig. 5, behind g.

The name given to lever P is "prism-lever," because at the side of its upper end is a little prism-shaped projection *p*, and the flat curved spring *r* combines its movements with I.

The name of lever W is "inversion-lever;" it carries regulating screws v^1 and v^2, the "contact fork" G, with two notches between its jaw at *k*, and terminates its arms in the contact screws *o* and *u*, Fig. 5. The click spring *h*, attached to the frame, decides its two positions, namely, the normal one when the click is in the lower notch, the second when *h* is in the top notch through the depression of G.

Besides the oscillations of the impulsor, the system of levers is moved by means of the motor A, and the *s* shaped flat spring *n*, attached to I at V.

In Figs. 2, 3, and 4, this motor carries a prismatic projection p', and a shoulder at S. In Fig. 5 these projections are shown as they actually exist, fixed on the side of the revolving disc K, the plane of which is so close to that of the levers that the prisms *p* and p' touch one another, and S pushes *n* at each revolution of the disc. In Fig. 3, p' is shown passing on one side of *p* and giving it a pull, in Fig. 4 passing on the other side and giving it a push. In the case of Fig. 3, the pull is followed by shoulder *s*, which, acting on *n*, throws I back against *m* (see Fig. 4). The bent spring *r* takes *p* in the same direction, and p' again arriving pushes at *p*, which causes the lower end to push W the other way at V^2, which raises fork G, and replaces the click on *h* in the lower notch, as in Fig. 2.

Thus we see that the lever P can occupy four positions—

1. The position to the left as in Fig. 2.
2. That shown in Fig. 3, when thrown to the right by the "impulsor."
3. That when caught and drawn against v^1 by the prism p'.
4. That when it rebounds under the influence of the push given by S to *n*, acted on by *r*, and by the final push of p' against *p*.

Next to describe the right hand portion of Fig. 5.

Above is the disc K already mentioned, which carries prism p'. It is called the *permutation* disc (see also I., II., III., &c., Fig. 6),

and as shown is cut out on the edge with notches covering more or less of the circumference.

Below is the support S′, which is insulated from the bed, and which carries on an axle the contact lever C, the horizontal arm of which, provided with contact springs, moves between *o* and *u*, the vertical arm of which is provided with a beak, H′, which is pressed against the edge of K by the spring r^3, regulated by v^4.

It is easy to see that when the disc revolves its protrusions and indentations act horizontally on the beak H′, and vertically on the arm C.

The next stage is to trace the electrial action on the mechanism.

A permutation relay with its impulsor is connected electrically with each of the five local contacts, see 1, 2, 3, 4, and 5, Fig. 1.

When no current affects the impulsor the fork G is horizontal, and the click is in the lower notch. The permutation disc K revolves steadily by the local clockwork. Its rotation moves arm C (Fig. 5), so that it rests on, or is separated from *u*, this makes and breaks a local circuit, but while G is horizontal no contact can take place through *o*. When the beak H′ falls forward into a notch, circuit is completed at *u*, this is called "disc contact." When a projection occurs on the edge of K, the circuit is broken at *u*, one pole of the local battery being connected to G, the others to C.

When, however, a current affects the impulsor, the lever system is thrown to the right, the prisms engaging as before described, and G is depressed, so that the click enters the upper notch at *k*. Immediately after the shoulder S touches *n*, and throws the impulsor back to its original position.

Only when the fork G is depressed, and when a projection at the same time under H′ raises C, does the latter make contact at *o*. This is called a "key-board contact." Thus there are 4 positions.

(1.)	G horizontal.	C depressed.	Contact at *u*.
(2.)	G ditto.	C raised.	No contact.
(3.)	G depressed.	C raised.	Contact at *o*.
(4.)	G ditto.	C depressed.	No contact.

At each completion of a revolution of the disc K prism *p*′

causes the prism lever to follow the impulsor (see Fig. 4), which acting on v^2 restores G to the horizontal.

Let us now follow the formation of the letter A, bearing in mind that there are five of these permutation discs on the same axis for each "communication."

The 1st and 5th keys are depressed at the distant station, the currents are sent from the distributer there along the line to the distributer at the receiving station, acting on relay E (Fig. 1) as they arrive. In this order the local batteries send a current into the permutation relays R^1 and R^5, through contacts 1 and 5. As described, the impulsors unlock, and the forks of 1 and 5 are depressed; then only, at one position of all the five discs will the printing circuit be completed, namely, when in revolving, their edges present a projection to the disc levers (K') of 1 and 5, and an indentation to 2, 3, and 4, see Fig. 6, and simultaneously will the letter A on the type wheel, which is moved by the same axle, present itself to the printing roller, and its impression be struck off on the paper.

Any one who has followed the description already given of the Baudot apparatus will now readily appreciate the system, how that the printing circuit is only completed for the impression of each character at the moment when the five discs influence the arms C to make contact with G in 30 combinations of five, brought about by the joint mechanical action of the discs K, and of the impulsors I and their adjoining levers.

It will be seen that there must be a relation between the movements of the limb on the distributer and the discs, and that the speed of revolution must be the same. The 5 prisms, p', on the 5 discs of the same receiver, meet the 5 prisms, p, on the prism lever at the same moment, and at that moment the zero point on the type is over the printing roller. At that instant, too, no current can pass, and the action p' on the prism levers of the displaced impulsors cannot be interfered with. This moment is called the "passage of the prisms," which, as shown, prepares the levers for the fresh group of positions required to form the next character. The distributing disc at the station is divided into quadrants, one for each communication, and the passage of the prism levers for

each takes place half a quadrant in advance of the passage of the distributing limb over the quadrant of the five contacts with which the impulsors are connected.

Although the mechanism which turns the limb Z (fig. 1) is independent of that which turns the axle of the permutation discs, and although Z only acts electrically on each set of impulsors during one quadrant of its revolutions, the work done by it to each set takes place within about 45°, in its revolutions, of the point at which the "passage of the prisms" is effected.

It is evident, that, if the revolving limb of the distributing disc at the sending station makes one revolution a second, it will pass over one quadrant in a quarter of a second, which then would be the time occupied in transmitting the series of currents necessary for each character, each of the series requiring one-fifth of the quarter second; and thus an irregularity of current on the line occurs from the different grouping of the five sending keys, which, as in Baudot, is of no importance.

We must imagine the circle in which the permutation discs revolve divided into 30 parts. Parts 29 and 30 (zero point) occur at the "passage of the prisms." When the prisms on the discs reach the second part, if the impulsors to form A have been unlocked, the local circuit is closed and A is printed; similarly, when they reach the third part and the combination is completed, B is printed, and so on to the 28th part, when Z is printed.

Again, to print V, the rubber Z, in its passage over the quadrant, has received on the line contacts *c*, *d*, and *e* (fig. 1), the currents from the three last keys. The receiving limb and the permutation discs continue to revolve to the point where the "passage of the prisms" takes place; the forks, C, of 3, 4, and 5 are pressed down by the action of the impulsors. In order that the printing circuit may now be closed, the permutation discs 3, 4, and 5 must present to their disc levers a projection, and 1 and 2 present each an indentation. This will not happen until the discs turn $\frac{23}{30}$ths further. When $\frac{22}{30}$ths are passed, the closing of the printing circuit has already begun by the pressing down of the forks of Nos. 3, 4, and 5, No. 1 remaining in its normal position, the circuit only being incomplete at No. 2. Immediately the 23rd

section is entered, No. 2 presents an indentation to its lever, and the printing circuit is closed and V struck off. Again, to print the letter H, we require projections of the discs to make contact at the depressed forks of Nos. 1, 2, and 3, and indentations to make contact at the forks of Nos. 3 and 4, which are normally placed. This can only occur at the 9th section of revolution, because in the

1st section,	discs	2, 3, 4, and 5	produce	no contact.
2nd ,,	,,	2, 3, and 5	,,	,,
3rd ,,	,,	2 and 5	,,	,,
4th ,,	,,	2	,,	,,
5th ,,	,,	2 and 4	,,	,,
6th ,,	,,	2, 4, and 5	,,	,,
7th ,,	,,	4 and 5	,,	,,
8th ,,	,,	4	,,	,,

If H had to be printed consecutively, the impulsions of Nos. 1, 2, and 3 would unlock each time against their prism levers, but the forks would remain depressed.

If we look upon the "passage of the prisms" as the commencement, we can understand how one letter is formed each revolution, because the forks to do this move at the beginning, and the currents destined to move the impulsors, immediately after the next "passage of the prisms," are emitted during the last quarter of the revolution,

As already said, a transmitter and a receiver consist of

5 keys, worked by an alphabetical key-board.
5 line contacts.
5 local contacts.
5 permutation relays.
5 permutation discs.
1 printer.

The apparatus exhibited at Paris carried four of these, but instead of every part being repeated four times, each group of five permutation relays serves two receivers, one impulsor being acted on by the currents transmitted from opposite quadrants of the distributer, but each impulsor acts against two sets of levers. The shoulder S shown in dotted lines on the same axis as the disc,

Fig 5, is the means by which the impulsors are locked in time to be acted on afresh by the currents transmitted by the contacts of the opposite quadrant of the distributer.

Herr Schaeffler divides the whole of the functions for the production of a printed character into three periods.

First. The emissions caused by the depression of the keys, the movements of the armature of the line relays at each station; the unlocking of the impulsors from their permanent local magnet, and the projection of the prism levers into the field of rotation of the prisms on the discs.

Second. The "passage of the prisms" and the restoration or locking of the impulsors, the movement of the inversion lever, and the depression of the contact forks.

Third. The rotation of the discs until the suitable projections and indentations are presented to the disc lever, the closing of the printing circuit and the impression of the type.

As this description pretends to be no more than an attempt to give the Telegraph Engineer an idea of the general plan and design of this wonderful mechanism, all details descriptive of, the key-board, the differences between the distributer and correcting mechanism of Schaeffler, and those of Meyer, the improvement in the printing mechanism, which otherwise is the same as Hughes, and the governor, are purposely omitted as being more the work of the text book.

Gray's Harmonic.

The Harmonic (including the way duplex) Telegraphs, now shown by Mr. C. C. Haskins, of Chicago, an old comrade in telegraphy since 1846, was the most original feature in the telegraph part of the Exhibition.

Its conception is suggestive, and its construction simple, contrasting in this last respect with the multiple type printers.

There are two examples of the system, and I shall proceed briefly to describe them.

The first is the combined Gray and Morse telegraph, already mentioned in a past number of our journal; the second is the Octoplex, worked on the harmonic plan in its simplest form.

In the first example, Mr. Gray has shown an application which is practically applicable to many telegraph lines. On one wire he uses the Gray's vibrating telegraph at the terminals, and adds several intermediate Morse Stations. (*Vide* Fig. 3, plate 5.) Thus, if we suppose a wire, London to Edinburgh, with Gray's instrument at those places, it can have Morse instruments on it at Cambridge, York, and Newcastle, as well as at the terminals, working between one another, without any interference with the communication between the terminals.

The circuit for all the stations is closed, the line battery being placed half at each terminal.

Let us first follow the work done at the terminal stations by the vibrating portion.

The vibrators (Fig. 1, plate 5) are the instruments used for producing intermittent currents, which the Gray's key at the stations sends to line. When the key is at rest, the vibrator continues to vibrate, but no wave currents pass to line.

It must be borne in mind that the working currents are of two kinds. One kind is solid, the other vibrating. The first is always in operation, except when the second is substituted by the closing of the Gray key.

The vibrator is a very ingenious instrument, and although described by Prescott I venture to present it again.

Between two electro-magnets of unequal power, is introduced the end of a tuning fork F (see Fig. 1, plate 5). The fork is fixed and carried at A. At B and C is a contact screw, adjusted only to touch the fork when it reaches them in its vibrations. When a local battery is completed through the coils of the magnets, the strongest on the left attracts the fork; immediately the fork touches B, the current is shunted, via D, out of the coils of the strongest magnet, and at once the attraction of the weaker draws it to the right. The shunt broken, the left again attracts, and so an automatic vibration is set going. Each contact at C completes the line battery, and the vibrations are transmitted, as already described, when the sending key is depressed in long and short series, to be received at the distant stations (through the key at

rest there) by the receiver, in which the fork, tuned in unison, vibrates in correspondence.

On this fork F (Fig. 3) rests a small light lever L, which vibrates too, but not synchronously; in other words, immediately the fork begins to vibrate the lever also commences, but with different rates of motion, so that practically the duration of a series of vibrations, long or short, in the fork, represents an interval of break of contact in the relay Y adjoining, and consequently a make of contact in the Morse, which is worked by the relay. We have thus an ordinary single working sounder circuit between London and Edinburgh.

At each of the intermediate stations already named, Cambridge, York, and Newcastle, a relay working a Morse local is introduced, as well as at the terminals, and worked on the ordinary closed circuit system.

When these stations are at rest the vibratory currents pass through the coils of the relay and the key.

When the key at one station is raised to signal, these currents pass again to line through a shunt with a resistance in it of about 6,000, obtained by a rheostat, R, so that while the relay armatures are all moved by the opening of the key, they are not influenced by the vibratory currents which continue travelling to the terminal through the shunt. In practice it has been found that this effect on the relay is only perfectly obtained when a condenser of about 2 microfarods is placed, so that one terminal joins the line before it is connected with the relay, and the other on the line side of the key (see Fig. 3).

It is evident to any one who has thought about the subject what a saving in local wires on our lines this increase in the capacity of a wire is capable of affording.

In the example I have described, we have the work of two wires distinctly performed by one; thus, we have a through circuit and a way circuit in one, and hence the system has received the name of the "Way duplex."

Mr. William Orton, President of the Western Union Telegraph Company, originated for the circuits thus, as it were, stolen from a wire, the happy name of "phantom circuit."

At present the greater number of our through wires in this country would be embarrassed by the limit placed on them by this system, and, it is in fact most applicable to lines of communication on which the circumstances are suited, namely, when the messages between the terminals amount to about 150 a day, and there are intermediate Morse stations.

But the more extended and truer application of Harmonic Telegraphy opens a startling field for the increase in the capacity of existing wires, which will infallibly, when carefully thought out and tried, revolutionise the means of communication in all countries.

It is the evidence of this discovery which marks the Paris Exhibition of 1878 as the commencement of a new era in telegraphic communication, and which will, some day, oblige the telegraph administrations everywhere to reconsider, their tarifs, the form of their apparatus, and the status of their employées.

Dealing daily with all the matters which have to be considered in providing for and improving the means of telegraph communication between place and place, I confess to having been fascinated with the vista of facilities which the comprehension of this subject put before me.

At this moment it can only be illustrated in the crudest way, but every practical telegraph engineer in this room will easily grasp what perfection it is capable of receiving.

The Octoplex harmonic telegraph means that on one wire four double lines of communication can be established; it further means that the eight stations may be situated where you please on the line.

To commence, let us examine the apparatus placed at two stations, between which it is desired to have eight communications.

We start again, with a closed circuit, having, say 50 cells Fig. 2), at each station. At each there are four vibrators V_1, V_2, V_3, V_4, and four keys joined up with the line battery in the following way :—

Each key, with its vibrator in circuit, is joined to a portion of the battery, so that the closing of the key short circuits so many cells; the effect being that a vibratory lessening of potential take

place, the rate of vibration being due to the note of the tuning fork in the vibrator. Thus, V_1 vibrates G sharp, and short-circuits 10 cells; V_2 A, and short-circuits 12 cells, and so on. The several undulations of potential so produced, pass direct to line, which however is first divided, half only going to the distant station, and half by an artificial line to earth locally. Between these branches, as in a bridge, the four Gray's receivers are placed in circuit, and are in consequence not affected by the outgoing currents.

The incoming currents divide on arriving at the bridge, and a portion of them, sufficient to vibrate the corresponding fork on the receiver, passes through, each undulation acting on the receiver which is tuned to be susceptible to the rate corresponding to its note.

It is obvious that the whole success of the identification of the currents, depends on the property of the receiving tuning fork, only to vibrate to the current which gives a rate corresponding with the note to which it is tuned, and to vibrate to no other. Thus each receiver should only sound when its corresponding vibrator at the distant station is connected to line in long or short series, by the depression of the sending key.

Each receiver is secured to a sounding box open at one end. This box is of dimensions decided on from experiment, and bears in its size and shape a relation to the note produced by the fork attached, the vibrators of which, though barely audible alone, are magnified in the box to a clear full tone, which intermits in long and short sounds in answer to the sending key, sounds which can be read by the ear with the greatest facility.

The following are some remarks made to me by Mr. Haskins, which accounts for the "way telegraph" not being shown to the meeting in perfect working order.

"In America local relays for the Morse stations are always used. Direct ink writers are comparatively unknown.

"The present set is imperfect. It is a makeshift.

"The system is unknown to English employés, and consequently will not work as perfectly as in the hands of experts in it.

"American relays do not average over 180 or 200 ohms, and are capable of very fine adjustment.

"The delay in getting the instruments from France has prevented their being shown in the best working condition.

"In America the line from Dubuque to Chicago had 17 Morse and two harmonic instruments in circuit.

"The line from Milwaukee to St. Paul, with six Morse and two harmonic, is 350 miles in length, working perfectly without interference.

"Between Chicago and Indianapolis the system has been used as a duplex, between Lafayette and Chicago, and between Chicago and Indianapolis as an ordinary Morse line."

It would be misleading to the meeting if I now neglected to point out that a good deal of what I have just been describing was brought out in this country so long ago as 1870, by our distinguished member Mr. Cromwell Varley.

In the first place, Mr. Varley published that year a description of a telegraph so constructed that current signals and wave signals can be simultaneously transmitted through the same line wire, and can be rendered sensible at the receiving station by separate instruments, the one sensitive to currents of appreciable duration, and the other to electric waves, or vibrations.

He took, for example, an ordinary Morse circuit, and proposed to place on it at several points transmitting and receiving instruments to produce and be sensitive to undulatory currents only. These instruments consisted of a vibrator, the currents from which were sent to line by a key in long and short periods; and, for receiving, a strained wire or tongue adjusted to vibrate in unison with the wave of current passing through an electro-magnet in its vicinity, called by Mr. Varley his "cymaphen."

At each point of the line where these wave signals are to be communicated and received, a condenser is attached.

Their communication to the lower side by the transmitter causes rapid charges and discharges, which induce on the upper side, and thence through the line, slight variations in potential, which do not affect the working solid currents of the ordinary communication.

These impulses correspondingly act in the condenser at the receiving station, and through it to the sensitive organ of the receiver, or "cymaphen."

At each harmonic station, Mr. Varley places on the line an

instrument called an "echocyme," not necessary to describe now, but which he states has the effect of dividing the line against the transmission of the wave currents, but not in the least impeding the ordinary solid ones, thus enabling the sections into which the line is divided by one or more "echocymes" to be used for local messages without interfering with the through traffic.

Mr. Varley also sketched out the notion of translating sound signals into Morse ones, as illustrated this evening by the apparatus of Mr. Gray.

The principle of action in the vibrators of Varley and Gray are different, and you will also have seen that the latter avoids the interposition of a condenser or an "echocyme."

Lastly, Mr. Varley has not omitted to point out that, if waves of, say, 200 and 850 vibrations in the second, be simultaneously transmitted and received, the voices of each can be distinguished on more than one "cymaphen" adjusted to answer to the rates of vibration, without the slightest interference between them.

In conclusion, and while thanking, etc., etc., etc., I venture to predict that, in view of the wonderful practical advances in telegraphic engineering, the era of really cheap communication, for public and private purposes, is not far distant.

The power of utilising each intermittent current, nearly 600 a second, which Baudot's and Schaeffler's apparatus illustrate, will cheapen the transmission of news and long despatches, and printed sheets of intelligence can emanate hourly from the telegraph.

The wave currents, distinguishable with nearly 46,000 undulations a second, and their interpretation in harmonic tones, will multiply our means for ordinary messages indefinitely, besides probably enabling us to use smaller aerial conductors; and possibly in a few years, by this means, telegrams will replace the great mass of written correspondence. While the telephone will permit us to hold conversations with our friends and business connections between all parts of the country.

The PRESIDENT: We have here brought before us matter requiring our close attention and deep thought. This invention of

Mr. Elisha Gray is, as Major Webber has said, one that is capable of immense development. I myself saw it in its rudimentary condition two years ago when I visited the United States, and had the pleasure of hearing an explanation of the apparatus by Professor Bell as it then stood. It is now advanced into a very different instrument, and I am sure we are very much and deeply interested, and very thankful to Major Webber for bringing this matter before us this evening. The apparatus will be worked, and I invite members to ask questions.

After a pause,

The PRESIDENT again rose and said: I see that members are hardly prepared to discuss this subject, and therefore, with your permission, we will adjourn this meeting; but first I will propose a hearty vote of thanks to Major Webber for his valuable and interesting communication, and, in adjourning the meeting, I invite members to come forward and examine the apparatus, and be prepared to discuss it at our next meeting. Mr. Haskins has given an immense amount of attention to the arrangement of these instruments, and I am sure you will join me in a hearty vote of thanks to him also for the ability he has displayed.

The meeting adjourned.

ABSTRACTS.

WINDING ELECTRO-MAGNETS.

BY W. H. PREECE, VICE-PRESIDENT.

M. E. Bisson recently submitted a paper to the French Academy, through M. Jamin, showing that if, in winding the wire on the core of electro-magnets, the wires, after completing a layer, be led back *straight* to the point of departure again, then to begin the next layer instead of winding it backwards so as to fill up the bobbin by layers wound in reciprocal directions, as is usually done at present, a gain of one-third in magnetic power will be obtained, every other condition being the same. This gain would be manifested either in sliding, pulling, or attraction at a distance.

To prove this rather promising result, I had a pair of bobbins wound with the layers of wire all in one direction, as indicated, to the same resistance as another pair wound in the ordinary way. These pairs of bobbins were interchangeable, both pairs fitting the same cores of an electro-magnet. It was observed that the same length of wire wound in the usual manner takes up less space than that wound in accordance with the new method, and winding in the latter way was found to be very difficult as compared with the former.

In no case was anything gained by the new method of winding, as claimed, but, on the contrary, the old method proved itself to be superior in all circumstances.

ON THE ELECTROMOTIVE FORCE PRODUCED BY THE PASSAGE OF WATER THROUGH CAPILLARY TUBES.

In the second volume of the Annalen der Physik und Chemie (Neue Folge), is a paper by Mr. J. W. Clark, of The Royal Indian

Engineering College, Staines, "On the Electromotive Force produced by the Passage of Water through Capillary Tubes," the result of which may be shortly stated as being:—

1. That the narrower the capillary tube through which the water is forced the greater the electromotive force so produced.

2. The electromotive force is independent of the length of the tube when of very small bore, but with tubes of larger bore it has a smaller value for long than for short tubes.

3. By covering the inner surfaces of the capillary tubes with different substances (*e.g.*, shellac, silver, fat, wax, &c.) different values for the electromotive force are obtained, which agree with the investigations of Quincke on "Diaphragmen ströme."

4. Time causes a remarkable diminution in the electromotive force, which takes place whether the water is allowed to flow continually through the tubes, or whether they are merely kept filled with water. By heating with sulphuric acid, and then with distilled water, the electromotive force can be restored to its original value.

5. The seat of the electromotive force appears to be the common surface of liquid and tube. The electromotive force thus produced is considerable. With an elliptical tube, of which the axes measured 0·2981 and 0·1380 mm., and a pressure of water equal to 1285 mm. of mercury, the electromotive force was found equal to 1·707 (Daniell = 1). This value, however, fell to 1·308 in about 120 hours.

For a description of the experimental details and of a modification of Kirchoff's form of Thomson's Quadrant Electrometer, with which the measurements were made, we must refer to the original.